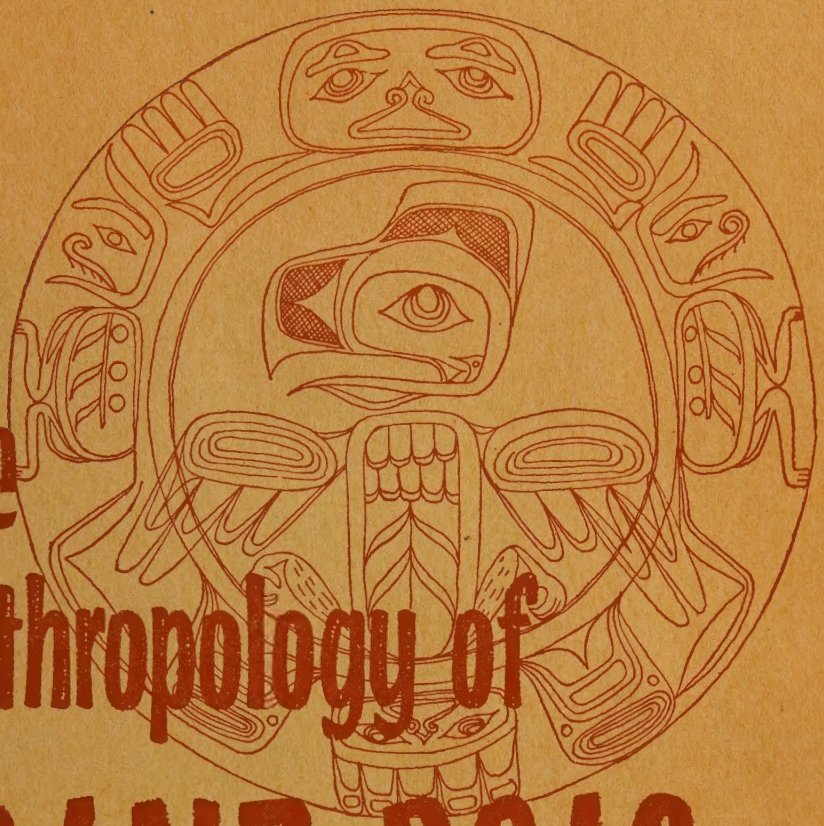

# The Anthropology of FRANZ BOAS

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*EDITED BY WALTER GOLDSCHMIDT*

*Memoir No. 89 of the American Anthropological Association*
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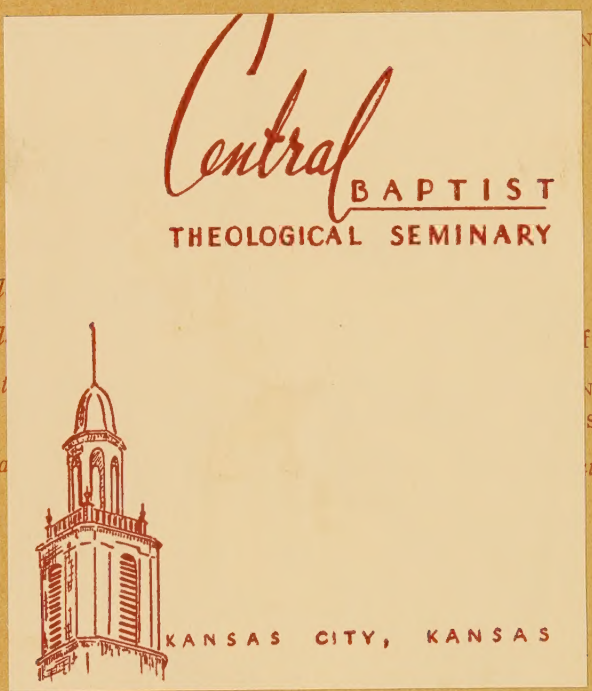


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THE AMERICAN ANTHROPOLOGIST

# THE ANTHROPOLOGY OF FRANZ BOAS

## ESSAYS ON THE CENTENNIAL OF HIS BIRTH

EDITED BY
WALTER GOLDSCHMIDT

*Sponsoring Editors*
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# *Preface*

A. L. KROEBER

TWO years ago, Walter Goldschmidt came to Berkeley to confer with Lowie and me on a prospective cooperative Memoir on Franz Boas, to commemorate the centenary of his birth. Goldschmidt decided favorably on the undertaking, with Lowie and myself agreeing to stand by with moral sponsorship rather than making substantive contributions additional to our papers on Boas a year or two before.

I have read in manuscript all the articles prepared for this volume. They constitute a notable collection, which will go far to define the relation of Boas to the development of anthropology and his place in science.

It is surely significant that natural scientists in general have never questioned the stature of Boas and the importance of his massive contribution, but that among anthropologists there have been occasional doubters or cavillers and more who have not been influenced by him or who have ignored his work.

The cause was, on one side, the heterogeneity of anthropological activity, which at first stemmed largely from both natural history and from humanist interests, but later increasingly from social science motivations and influences. On the other side, Boas' own peculiar life history and personality contributed. Of the papers in this volume, those of Kluckhohn and Prufer and of Marian Smith explicitly recognize the first factor, Melville Jacobs' admits it from a special angle, and in most the others, such as Jakobson's, awareness crops up or is just below the surface.

The great bulk of Boas' anthropological output is constituted by grammars, corpuses of texts, assemblages of tales, and ethnography—much of the last in the native language, whether it present enumerations of custom or case histories. Altogether, these are the kinds of data that are dealt with in the humanities, except that the languages and cultures involved are Eskimo, Kwakiutl, Tsimshian, and Chinook instead of Classical and European; and that they are dealt with because they are part of the total world of nature and not because of any supposed spiritual excellence or superior worth.

However, Boas' university training was in mathematics and physics, and his thesis, on the color of water, lay in the latter field, with a sidelong nod to geography, into which he had been attracted by Theobald Fischer. He had no formal training whatever in linguistics or philology, except for the Latin and Greek paradigms which he had learned in the Gymnasium. It is a tribute to Boas' intelligence, tenacity, and energy that his achievement in language was no whit inferior to that in other fields, although his knowledge of it was wholly autodidact and linguistics has the repute of being a highly special and technical subject.

Hardly had he received his doctorate when he set all efforts in motion to go on a geographical expedition. The trip to Baffinland yielded abundant cartographic observations, but far more important was the human by-product on the Eskimo. From this first contact with immediate cultural and linguistic phenomena, Boas became an unwavering anthropologist.

I believe there can be no doubt Boas wanted to deal with human rather than inorganic phenomena as soon as he found that it was possible to deal with them scientifically. This he could do through the principle that context is always part of the human phenomenon—a profound and original insight which was not taught him by the practice of physics, which indeed progresses rather by temporarily isolating the phenomenon from its context. It is perhaps Boas' fundamental contribution to science and civilization: to treat human data with rigorous scientific method on a scale unattempted before.

It is indubitable that science was his religion. He called his early convictions materialistic. Science could tolerate nothing "subjective"; value judgments—and by infection even values considered as phenomena—must be absolutely excluded. That is why he had no inclinations to become a humanist or historian. The fervor of his devotion to rigorous scientific method was perhaps the greater because German natural science was itself young. It had begun its active productivity definitely later than French, British, Dutch, Italian, and Swedish science; for the great German idealistic philosophy which was in culmination seems to have blocked the notable development of science in Germany until about 1800.

Given the combination of Boas' basic natural science adhesion and his predilection for human materials, it was almost inevitable that he should concern himself with culture, for culture—including its semi-autonomous province of language—is precisely the phenomenal regularities of human behaviors.

It is also significant that Boas showed an early interest in Fechnerian psychophysics—an attempt at a psychology within a rigorous physical framework; but he soon realized the limitations of this approach as against the wider potentialities of cultural analysis. Wundtian psychology, and that of Wundt's American pupil Cattell, his friend and colleague at Columbia, he was tolerantly respectful to; but psychoanalysis he would have no traffic with. As an antispeculative contemporary of Freud, Boas could hardly have been expected to see that the total Freudian myth, for all its unprovability, might serve as a model for provisionally construing much of human personality in totality and in context. Yet Boas presumably realized that the science-imitating method of psychological analytical experimentation was yielding only scant returns from its intractable material.

Culture is not only the regularities of human behavior, it is the product of human behavior, as it also becomes its channel and form; but it is not, in itself, human behavior. It is at this point that social science—now preferring its adult name of "behavioral" science—entered into anthropology. Its interest lies precisely in the interrelations of persons, in their behavior. It accepts these dynamic interactions, of course, as occurring in and framed by social and

cultural situations, as well as in turn influencing the situations. But its view is focused on the interpersonal behavior, not on its stream of products.

This stream of social science thought passed Boas by without much affecting him. He was nearing seventy, and had in hand much of his work to finish; if he was to be distracted, fighting Hitlerism and hate was the nearer job.

Thus I see natural history and science and humanistic study as the main factors that actuated Boas' thinking, as I see them also in the older anthropology of which Lowie and Sapir and I were part. Before Boas, in England, Tylor was certainly a thorough natural scientist in orientation—and, like Boas, his genius seized upon culture as that aspect of humankind most readily susceptible of generalizing phenomenal treatment. How far Tylor's Quaker origin permitted humanistic indulgence, I am not wholly clear about; though it certainly allowed full intellectual mellowness and tolerance. In any event his successor Frazer, with his antibody Andrew Lang, grew up on hallowed classic soil before he entered Exotica.

Contrariwise, social science was sired by eighteenth-century *philosophes* and their successors, and mothered by hope of practical applicability. It has considerably and consciously patterned itself after natural science; of which trend the name "behavioral" is a fair enough expression in a maturer phase. But even behavioral science is still often ameliorationist, taking utility for granted, is definitionist more than phenomenally directed, and, outside of anthropology, it remains ethnocentrically weighted if not aligned. University administrators have found it convenient to class anthropology in the social sciences; and there is a whole younger generation of us who accept the label and know no different. It does not work out too badly: we all live together amicably and comfortably, if often also somewhat confusedly; and I suspect the condition has come to stay with us for a span or longer.

But there are many younger anthropologists in America, and perhaps all but a few of the self-styled social anthropologists in Britain, and I am not too sure about some in France and Germany—in whose thinking Boas does not yet figure, because they assume that anthropology really is just another social science and who therefore do not know what the anthropology of a generation ago, on which that of today rests, was about. They are immigrants from different antecedents, premises, and orientations. It is for them that this volume on Boas is potentially most significant, as it is illuminating to those interested in the wider stream of intellection characteristic of our civilization.

June 5, 1959

# *Introduction*

WALTER GOLDSCHMIDT

THIS volume is a memorial to Franz Boas on the centenary of his birth. We believe it a fitting monument, and one of a character that he would have appreciated and understood. Out of the cold analytic facts of Boas' own work emerges the image of the man himself, and through the man, the discipline of anthropology which he in turn did so much to shape.

That the image of Boas should appear so clearly in these papers is something of a surprise, though perhaps it should not be. Each author was requested to present his materials analytically, and to avoid the polemics that have on occasion marred the discussion of Boas' work over the past few years. The request was inspired by the belief that anthropology was old enough to have a history and mature enough to evaluate its parental figures with detachment. Ultimately, it is his work that counts for us, for it has in greater or lesser degree shaped all of us in our academic endeavors. It is a pleasure to realize that our request was interpreted by the authors in Boas' own spirit. For certainly he insisted, in his own and his students' fieldwork, that the data themselves are the important matter, and that only through the recording and examination of the data can the culture emerge. Certainly, too, he avoided evaluation.

It was never my privilege to meet Boas personally, though I have known him as an intellect through his writings, and as a personality through his legend. These essays make me feel that I know him as a being and that I can understand the character of his influence upon anthropology.

Much in these essays documents and adds detail to Boas' characteristics which are generally known. His breadth and wide-ranging intellectual curiosity, which led him into such disparate aspects of the human scene as the statistics of bodily growth and the characteristics of primitive mythology, are documented by the very range of subject that these essays had inevitably to cover. Even here we have not managed to reach the full scope—for example, we were unable to obtain an analysis of his important contributions to the field of primitive art. Certainly, it was his personal range of interests, his concept of the discipline, more than anything else that has preserved in this country the unity of the anthropological sciences—with far-reaching, beneficial consequences for American anthropology.

Also well known are Boas' background in mathematics and physics and his thorough German education, which gave him his empirical bent and his insistence on the collection of facts and his distaste for facile generalization. Some of the details of this background have been set forth by Kluckhohn and Prufer in their biographical sketch of his German years. The implications of that background are documented throughout the volume: by Mead in showing his careful, internal analysis of cultural phenomena; by Tanner on his

analysis of bodily growth; by Codere on his treatment of Kwakiutl ethnography, and by Jacobs on his concern with mythic details. This empiricism, this concern with fact, with detail, with preserving the record, Boas transmitted to his students and to anthropology. It is so major an element in anthropological thinking that the term armchair anthropologist is one of opprobrium, and two generations later we still insist on fieldwork as a requisite to any claim for anthropological competence. It need not have been thus; it was not in the same sense true of contemporary anthropology in Europe (though it has become so), nor was it true of American sociology until quite recently.

Boas' liberalism is also known and appreciated. His distaste for racism and for any kind of prejudice was clear. His attitudes were heard in public places; he spoke out on matters of public concern and was willing to suffer the consequences of an unpopular stand. That liberalism which recognizes human dignity and the essential equality of mankind appears sometimes in unexpected places as an element in his thought. We see it in his studies of human growth, where, as Tanner shows, Boas insisted on the potential worth, the ultimate equality, of the retarded boy; we see it as well in his expressed recognition of the essential worth of primitive man, and in his recognition of racial equality. It suffused his entire thinking. This liberalism has been stamped on the anthropological discipline, so that it seems a natural characteristic of the field. But it had not—at least to the same degree—been characteristic of anthropology in the 19th century, nor was it characteristic of such sister disciplines as psychology and economics early in the twentieth. But Boas was a human being—a whole man—and these very virtues had their limiting effects. It seems to me that these limitations have also, to a remarkable degree, been stamped on American anthropological thought. They too are documented in these essays. There is a lack of concern with the person, with the individual in his interactions—a lack of empathy. It is this quality that makes it possible for Marian Smith to say that his work has "a curious detachment which is slightly repellent to social scientists absorbed in people." It is curious that this quintessentially liberal man should have had so little understanding of the individual. It is as if his belief that all people were equal rendered them all the same.

This characteristic suffused his field method, so that he could make the assumption, again according to Smith, that "any active participant in a society must be thinking and generally behaving in ways appropriate to that society." The assumption has a measure of validity for the homogeneous primitive systems that anthropology studies, but it is fully valid only for those kinds of tasks that Boas viewed as appropriate for anthropological enquiry: namely, the traditional, the customary, the normative. Myth, ritual, and language were his areas of emphasis; Helen Codere finds that "the outstanding feature of Boas' Kwakiutl and Northwest Coast work is the emphasis upon the symbolic aspects of culture." Thus, he studied precisely the kind of material that can be had from "any active participant" with the greatest degree of reliability. The same characteristic is evidenced in Boas' approach to mythology, as Melville

Jacobs shows, in his concern with the accurate text as representative of the cultural inventory, and his failure to examine style, the situational context of story-telling, or the psychological meaning of the tale to the teller and his listeners. Boas' methods of work could not handle these concerns, which is to say that they were not part of the scene to which Boas' anthropology addressed itself.

When we are in the field, I am sure that most of us wonder how it feels to be a participant in the culture we are studying—to have multiple wives, say, or to undergo initiation. One feels that such emotions were rarely evoked in Boas: he would address himself to the reasons for giving a potlatch, but would not have wondered how he himself would have felt about giving one. To Boas, anthropology was not concerned with the individual qua individual; with man's loves and hates, with his endeavors and frustrations. Being unconcerned with the individual, he did not see society as an interaction among individuals, loving and hating, manipulating and conniving. To Boas, anthropology was the study of the rules of the game, not of the game itself.

In no sense is this a criticism—no man of stature can be all things, and certainly Boas was many. Rather, it is set forth here because in this too Boas placed his personal stamp on the discipline. Anthropology—in its ethnological aspects—started out as the science of custom, despite its claim to being the science of man. In America it is still very much that. The traditional ethnography sets forth customary procedures, and even today it is not easy to find examples of the distinction between what informants say is standard practice and the actual practices of everyday life in the tribe. This perhaps is a major distinction between American and British ethnographic techniques. It is functionally relevant to such trans-Atlantic disagreements as the use of selected informants, the learning of the tribal vernacular, the uses of direct observation, and the necessary duration of a field trip. More importantly, it lies behind the essential distinction between the historical or cultural point of view and the functional or sociological.

As anthropology in America was so largely shaped in the image of Boas, so this monument to Boas offers us a mirror of that anthropology. To know ourselves is to know our past, and the papers here show us the form of that past.

# *Influences During the Formative Years*[1]

CLYDE KLUCKHOHN
OLAF PRUFER

BOAS, Durkheim, and Simmel were all born in the same year. Henri Bergson, John Dewey, and Havelock Ellis were born in the following year. Freud was born in 1856, and Max Weber in 1864. Thus cluster within an eight-year span the birthdates of a surprising proportion of those who have had a tremendous impact upon 20th century thought about man in society. It was also a period of intellectual events that either had very broad consequences or were of considerable moment in the history of anthropology. Take the year 1859 alone. This marked the discovery of spectrum analysis, the acceptance by Lyell and Prestwich of the findings of Boucher de Perthes, and publication of the following:

| | |
|---|---|
| Darwin: | *Origin of Species* |
| Marx: | *Critique of Political Economy* |
| Virchow: | *Cellular Pathology* |
| Littré: | *Paroles de Philosophie Positive* |
| Bain: | *Emotion and the Will* |
| Mill: | *Essay on Nature* |
| Whateley: | *Lessons on the Mind* |
| Waitz: | *Anthropologie der Naturvölker (Vol. I)* |
| Bastian: | *Ein Besuch in San Salvador, ein Beitrag zur Mythologie und Psychologie.* |

Quite apart from the historical record, an examination of major trends in the writings of the eight eminent men born in the 1856–1864 period would itself indicate two main currents in Western thought during their formative years. The one which shaped Boas was dominantly empiricist, positivistic, and "scientific," though it included two distinctive facets which will be discussed later. The other was more philosophical and speculative. It may broadly —and loosely—be characterized as "continental-Hegelian," and produced Max Weber and other neo-Kantian social scientists. And yet, having said this, it must be observed that some passages on culture in Hegel's *Philosophy of History* do not seem at all alien to central currents in Boas' thinking. Very possibly there was little or no direct influence from Hegel. Rather, one may speculate, it was a question of the *zeitgeist* in Germany at the relevant period.

Let us restrict the examination largely to the German-speaking world and to the first 25 years of Boas' life. Neither the task of sketching the general atmosphere nor that of tracing specific influences upon Boas have proven easy. We have read or consulted a vast quantity of literature, but it is hard to produce a summary both firm and succinct. Nor have we been able to find an intellectual history of Germany in the mid-19th century which was really use-

ful for our purposes. To be sure, a few things are clear enough. The works of Kant and Hegel dominated the philosophical scene. Goethe was still the culture hero of intellectual Germany, and he had an impact on the young Boas as he did upon the young Freud. Nevertheless, the pantheistic, somewhat mystical monism of *Naturphilosophie* which flourished in Germany from about 1794 to 1830, and of which Goethe was one of the pioneers, was under heavy attack (Ziegler 1899:322–7). In 1845, Du Bois-Reymond, Brücke, Helmholtz, and Ludwig had formed the Berlin Society of Physicists to destroy the vitalism of Johannes Müller. The "speculative physics" of Schelling (cf. Ziegler 1899: Ch. 2; Honigsheim 1954) was in bad repute. Helmholtz and Fechner were endeavoring to transform psychology into a natural science. Yet, as Jones (1953:43) points out:

> "Unity of science," "science," "physical forces" were not merely directing ideas or hypotheses of scientific endeavor; they became almost objects of worship. They were more than methods of research—they became a *Weltanschauung*.

Starting in 1863, Haeckel popularized Darwinism in Germany (Irvine 1955:116–7). Honigsheim (1958:4) correctly says:

> Als Simmel stätete, herrschte im Berliner unoffiziellen Geistesleben eine Kombination von Positivismus, Naturalismus, Evolutionismus und englischen soziologischen Darwinismus.[2]

German intellectuals, with notable exceptions, had become politically conservative (Lowie 1954:108). Other symptomatic historical details, possibly more directly relevant to Boas as an anthropologist, may be mentioned. The works of Alexander von Humboldt (died 1859) and of other earlier travellers were still widely read and discussed. Bastian was travelling and writing upon his travels between 1850 and 1880. In 1857 the Neanderthal remains were discovered. In 1879 Virchow worked with Schliemann at Troy.

So far as Boas specifically is concerned, the trail is elusive. His bibliographies are generally sparse or devoted to documentation of concrete points rather than to making clear the sources of his basic philosophy of science or broad intellectual influences. It is clear from his later writings that he admired Galton and Pearson and drew from Ratzel, Hahn, and others. But if we take Boas' "formative period" to be that before he made his first field trip to the Eskimo, it is doubtful if, before he left for Baffinland, he had read much of these and others whom he cites, though his unpublished correspondence makes it clear he did wide reading in Berlin in 1882–83. Ratzel (cf. Lowie 1943a), however, had just published his first major work (1882) and Hahn did not publish a major work until 1896. The Ratzel-Hahn influence may have been mediated through Lanfer. At any rate, Herskovits and others of Boas' students are emphatic as to the prominence of Ratzel in Boas' teaching. We have encountered no evidence as to when Boas first read Tylor. Boas cites several times (and apparently with at least partial approval) Andree's *Ethnographische Parallelen* (1887), but only in works published after he came to the United States.

## BOAS PRIOR TO HIS UNIVERSITY YEARS

The Boas correspondence in the American Philosophical Society makes evident how early some of his basic bents appeared. For example, in a letter to his sister written on March 12, 1870, he says that after he has taken his doctorate of medicine he would like to go on an expedition to the North or South Pole and travel to Australia or Africa. A letter of June 20, 1871, says that he wants to do research in "all the unknown lands" and:

> Neue Völker und deren Sitten und Gewohnheiten möchte ich kennen lernen, auch die schon bekannten Galla-, Banda-, Kaffern-, Hottentotten-Völker. . . .

At the age of thirteen Boas was already talking like an anthropologist!

Through the kindness of Helene Boas Yampolsky, we have been able to read the extensive *curriculum vitae* which Boas wrote on his graduation from the Minden gymnasium. This is a most illuminating document, both for the facts it contains and for the revelations of the extent to which the interests and qualities of mind associated with the mature Boas were already explicit and well established in the young Boas.

Boas mentions or discusses his studies of botany, geology, mathematics, geography, English, biology, mineralogy, Greek, Latin. With his mother's help and encouragement he engaged in nature study on his own. At one point he had a private tutor in botany. He read widely in literature. "I had soon devoured most of Schiller's writings and knew much by heart." Goethe's *Sorrows of the Young Werther* apparently impressed him, but he notes elsewhere that he did not care much for novels. Of *Faust,* however, he says, "Day and night I thought of little else." He also comments upon readings that were assigned to him: Shakespeare and the Greek and Latin classics. Homer was his favorite among the poets. He liked Thucydides and Herodotus best among the prose writers. "In Caesar I was interested in the history of the Celts and in what was written about their culture and religion." He tells us that he approached the writings of Alexander von Humboldt "with great awe." He also speaks of his own desire for foreign travel, remarking that at one time his greatest interest was in the tropics and specifically in Africa.

He states that at the gymnasium geography was his favorite study, though he mentions that he was not in the least interested in political geography. He also observes: "I was most interested in cultural history and was very sorry that it was very meagerly or not at all touched upon in school." On the other hand, his enthusiasm for mathematics and the natural sciences is evident:

> Mathematics interested me so much that I soon lost sight of my original purpose and studied mathematics for itself alone. . . . Since my earliest youth my greatest desire had been to study natural sciences. When I studied mathematics and physics it was these two that attracted me most.

In the following quotations there emerge points of view that were central with Boas to the end of his life:

> These books had one weakness. They caused me to pay attention to single details and did not give me an understanding of nature as a whole. . . . The fact that I made a

herbarium of various kinds of mosses shows how accustomed I was to notice details only. Perhaps it also shows I had the tendency to make comparisons. . . . I began to turn away from botany as mere collecting, and naming the plants no longer gave me pleasure. . . . I learnt that true science does not consist in describing individual plants but in understanding their structure and life and in comparing all the different groups of plants with one another. . . . I had already discovered that I preferred comparative science to the descriptive.

## BOAS IN THE UNIVERSITIES[3]

It is regrettable for the history of science that Boas' *curriculum* in his doctoral thesis is so brief. He indicates that he studied one semester at Heidelberg, four at Bonn, and two at Kiel. He gives the names of 16 professors with whom he worked, but only their surnames. We have identified only the following with reasonable certainty:

| | |
|---|---|
| Robert W. Bunsen | Chemist, Heidelberg. Inventor of the Bunsen burner. |
| Moritz Cantor | Mathematician, Heidelberg. Author of *Mathematische Beiträge zum Kulturleben der Völker.* |
| Rudolf Clausius | Physicist, Bonn. Well known for his work on kinetic gases and thermodynamics. |
| Theobald Fischer | Geographer, Bonn and Kiel |
| Johann v. Hanstein | Botanist, Bonn. |
| Lazarus Fuchs | Mathematician, Heidelberg. |
| August Kekulé v. Stradonitz | Eminent physicist and chemist, Bonn. |
| Arnold v. Lasaulx | Geologist and mineralogist, Kiel. Author of a small book called *Sicilien, ein geographisches Characterbild.* |
| Franz v. Leydig | Biologist, Bonn. |
| Gustav Karsten | Physicist. |
| Karl Möbius | Marine biologist. |
| Rudolf Lipschitz | Physicist and mathematician, Bonn. |
| Gerhard vom Rath | Mineralogist, Bonn. |

There are three biologists. Physicists, mathematicians, and chemists predominate, and some of them were men of eminence. In a letter to his mother, dated April 24, 1877, he says that the previous day he attended his first lecture with Bunsen:

> Als ich zun ersten Male zu ihm hinging, war mir ganz wunderlich zu Muthe, er war die erste wissenschaftlich Grösse, die ich mit meinen Augen sah und nun gar einem solchen Meister zu Füssen zu sitzen und seine Worte zu hören, ist ein noch viel schöneres Gefühl.

However, not all of his professors received equal adulation. In this same letter and in subsequent ones, he complains of Fuchs' lectures. In a letter to his mother of July 8, 1877, he says he has no desire to listen to physics lectures at Heidelberg but prefers to go to Bonn to listen to Clausius because he could hardly hope for anything better anywhere. It is worth noting that vom Rath was a world traveller and that Möbius wrote a paper utilizing ancient and modern Japanese sources which is somewhat reminiscent (as is von Lasaulx'

little book) of Theobald Fischer's approach. It seems plausible that Boas' early interest in geography and cultural history may have received some reinforcement from Cantor, von Lasaulx, Möbius, and vom Rath.

From the letters we learn of teachers not mentioned in his *curriculum* and of other activities. At Heidelberg he audited lectures of the famous philosopher, Kuno Fischer. At Bonn he studied with the ("hiesigen") astronomer Eduard Schönfeld and the mathematician Hermann Kortum. At Kiel he attended Erdmann's lectures on philosophy. A letter to his father dated November 9, 1877, states that he was attending 17 hours of lectures per week in mathematics, crystallography, theory of electricity, integral calculus, physics, theory of heat, and geography. Later he heard 26 hours per week. Additional subjects included botany (which he disliked on account of von Hanstein, who taught it), history of philosophy, statistics, and a one-hour per week series of lectures on a polar expedition. At Kiel, in addition to the natural sciences, he studied economics, geology, philosophy, and much geography (courses on Africa, Asia, and other areas; history of geography). At Heidelberg Boas joined the mathematics club and gave a talk to professors and students on the "Lehre von der Ellipse." On July 23, 1877, he wrote his parents that he would like best to study physiology but that this would take too long; therefore he will do physics. At Bonn he bought 40 volumes of Herder. He also mentions that he owns Humboldt's *Cosmos*.

To speak of less serious though interesting matters, the letters suggest that at Heidelberg and Bonn Boas enjoyed student life (drinking, "culture"—in the Matthew Arnold sense) and spent money rather freely. Only at Kiel does he appear to have become very serious. Both at Heidelberg and at Bonn he was imprisoned for student pranks. Letters of July 30, 1877 (Heidelberg) and March 18, 1879 (Bonn) speak of his participation in duels, and in both cases he was wounded.

He received *summa cum laude* on his thesis and *magna cum laude* on his doctoral examination. A letter to his father of July 24, 1881, goes into some detail on the examination questions. Fischer asked him about such matters as the geography of New Zealand, ocean currents, the agricultural civilizations of North America and China and the conditions for their development, the requirements for culture development in Siberia, the ethnography of North America.

Of all of Boas' university teachers only Fischer emerges as a distinctly personal figure. On present evidence we can hardly say more of the others than that Boas followed such and such lectures or, in some cases, expressed such and such sentiments concerning them. We have not found it possible to trace specific influences of other than a trivial sort. (Thus, Boas cites Bunsen in his doctoral thesis but only on factual matters.) On the other hand, Mrs. Yampolsky recalls that her father spoke of both Bunsen and Theobald Fischer with admiration.

Fischer is first mentioned in the letters early in Boas' second semester at Bonn, when Boas mentions that he is taking four hours a week in geography

with him. It has several times been stated in print that Boas "followed Fischer" to Kiel. This may be the case, but a letter of September 28, 1879, from Boas to his mother raises some doubt on this point. First, it seems to imply that the instigation for the move to Kiel did not come from Boas himself but from his family. Second, Boas says explicitly that he can learn enough in Kiel in one semester and probably will not return there. Boas' earlier letters from Kiel are preoccupied with the natural sciences rather than with geography. But by September, 1880, there begin to be frequent mentions of Fischer and his wife and indications that Boas' parents and Fischer are in communication. In October of that year Boas writes home that so far as his examinations are concerned, he has "really no fear of maps and Fischer," and adds: "bei Fischer ist nur das unangenehm, dass er noch nie geprüft hat." In November, 1880, Boas communicates that Fischer has strongly advised him (after completing his military service) to study with the geographer Wagner at Göttingen. Subsequent letters speak of Boas' being invited by Fischer to dinner and of greetings being sent back and forth between the Boas and Fischer families. By 1883 Boas refers to Fischer as "Theobald" (letter of January 13). After Boas had come to the United States, a letter from Fischer to one of Boas' relatives (seen through the kindness of Mrs. Yampolsky) urges at length that Boas make his professional career in Germany (in spite of being a Jew) rather than in "the rotten Yankee United States." Boas and Fischer remained in contact until Fischer's death in 1910, and each referred in his writings to works of the other. It is interesting to note that at the presentation of the Boas Anniversary Volume in 1906, Fischer wrote to the editor of the volume that

> . . . auch ich ihn [Boas], der von der Physik zur Geographie gekommen war, auf die historisch-ethnographische Seite des Fachs hinweisen zu müssen glaubte (Laufer 1906: VIII).

Here, then, is confirmation that Fischer was at least partly instrumental in directing Boas' interest toward anthropology.

## 1881–1883

We could wish to know much more about the events of these years, but we can repeat some established points and add some hitherto unpublished information. Boas completed his military service. He worked in Berlin writing papers on psychophysics and doing library research on the Eskimo area. A letter to his parents of October 24, 1882, says that on the previous Saturday he went to a meeting of the Berlin Anthropological Society. There a Dr. Reiss (whom we have not been able to identify) introduced him to Bastian and Virchow. He discussed his plans with Bastian, who gave him "einen guten Rat, was ich alles noch lernen müsste . . ." Later letters speak of frequent attendance at the meetings of the Anthropological Society. Bastian (letter of January 13, 1883) agrees to arrange with Virchow for Boas' instruction in anthropometry. Letters later that month speak of the good advice received

from Fischer and Bastian on the Eskimo expedition. The letters of this month also deal repeatedly with possible schemes for financing the trip and with his parents' opposition to the venture.

## FURTHER CONSIDERATIONS

What we know of Boas' life prior to his departure for Baffinland is instructive both as regards various details and certain general trends. His interests alike in natural science and in geography and cultural history are continuous from his teens. From his gymnasium and university teachers in mathematics and the natural sciences came not only certain skills which he was to utilize in his work in physical anthropology and statistics but also, presumably, a reinforcement and sharpening of a pre-existing disposition toward habits of mind which were sceptical, rigorous, and respectful of data.

There are many tantalizing gaps in the data, but we shall mention only two. Boas owned a set of Herder, but how much did he read? It is tempting to infer a linkage. Herder stressed the social nature of man, and adhering to observation as opposed to speculation, he attempted to "show what man was on the earth in general and in every region in particular" (Penniman 1935:48). In one place Boas writes:

> . . . Herder's "Ideen zur Geschicte der Menschheit" in which perhaps for the first time the fundamental thought of the culture of mankind as a whole is clearly expressed (1904:514).

And how early did Boas read French scholars? He had relatives in Paris, but the correspondence does not indicate that he visited France. A letter to his sister written October 6, 1870, contains a French sentence (with a grammatical error). He cites French sources in his doctoral thesis. Apparently his first published reference to a French social scientist was to Comte, and it appeared in an 1887 paper (1940:642). Influence from Tarde certainly occurred after Boas was established in the United States (Kroeber 1956:154–5).

Boas early developed a liberal political attitude and strong convictions on political matters. Sources on family atmosphere and manifestations throughout his life have been treated by Kroeber (1943), Lowie (1947), Herskovits (1953), and others. Full utilization of the materials in the American Philosophical Society should provide data for a rich study of the intellectual and personal influences that shaped Boas' political thinking.

In this and in various other respects Boas was undoubtedly influenced by the position of Jews in 19th century Germany (cf. Dubnov 1929:38, ff., 325). The conservatives were the bulwark of the discrimination against Jews in academic and other circles. It seems evident that one of the many things that made Virchow as much of an "idol" as Boas ever permitted himself was Virchow's stalwart opposition to all forms of anti-Semitism (cf. Lowie 1954: 107, 316). The correspondence repeatedly shows how central this problem was in Boas' formative years. A letter of October 6, 1870, records a poignant inci-

dent. The letters from Kiel are particularly full of accounts of unpleasant activities among the student body, and of gross personal behavior.

We are not competent to write an essay on the total intellectual climate in the German-speaking world during Boas' formative years. Reference has already been made to Goethe, to the famous philosophers, to the tradition of radical empiricism which influenced Boas so deeply. One could add the familiar points that there was constant discussion of scientific laws, stress upon the unity of science, and considerable talk in some fields (e.g. geography) of the value of interdisciplinary approaches.

However, we can present something on the work and scientific outlook of certain men who are known to have had an impact on Boas and with some of whom he had personal contact. It would be surprising indeed if Mach had had no influence on Boas, but, since we have found no explicit indication, we must omit him. We also omit two psychologists who certainly did touch Boas' intellectual life—Helmholtz and his student, Wundt. Boas cites Wundt in his psychophysics papers published in 1882 and in later anthropological writings; he is known to have had an interview with him in 1885 on psychophysics, and the unpublished document suggests he had met Wundt earlier; Kroeber (1943:7) says: "He spent much time in his seminars, for several years, on Wundt; but it was the Völkerpsychologie, not the experimental psychology . . . that occupied him." Nevertheless, neither Helmholtz nor Wundt appears to have exercised as distinct an influence on Boas as did Fechner.

The remaining six men fall rather naturally into two groups which correspond, we believe, to a basic antinomy in Boas' thought. The first group (Humboldt, Ritter, Fischer, and Bastian) were dominantly empiricist and closely related to much of the current natural science tradition, yet their outlook stressed the holistic or "embracive," the phenomenological. As examples of a somewhat contrastive stream of influence we take Fechner and Virchow, though Helmholtz and (in part) Wundt and presumably most of Boas' teachers in the natural sciences would also fall into this second group. Their approach was more abstractive or "isolative," less concerned with phenomena in their totality. Though Humboldt, Ritter, and Bastian were certainly interested in "laws," the first group are predominantly particularists while the second seek abstract generalizations.

## THE "EMBRACIVE" APPROACH

*Humboldt.* Boas spoke of the "awe" with which he approached the reading of Humboldt and of how he very carefully prepared himself before venturing into the third part of the *Cosmos*. In his famous 1887 paper on "The Study of Geography," Boas speaks of Humboldt's "admirable works" and refers later to the *Cosmos* in a passage (1940:642) which we believe to be crucial to the essence of Boas' philosophy of science.

Alexander v. Humboldt (1769–1859) had an enormous influence on intellectual life in Germany in the 19th century and was a world figure (for ex-

ample he was invited by Jefferson to Monticello for several weeks as a "consultant" [Brann 1954:12, 16; see also Ackerknecht 1955]). Humboldt's general ideas drew much from Kant and Goethe, but what is most interesting for the present study is his view of science and nature as stated in the *Cosmos*. At the outset he states:

> In considering this study of physical phenomena, not merely in its bearings on the material wants of life, but in its general influence on the intellectual advancement of mankind, we find its noblest and most important result to be a knowledge of the chain of connections, by which all natural forces are linked together, and made mutually dependent upon each other . . . he who can trace through by-gone times, the stream of our knowledge to its primitive source, will learn from history how, for thousands of years, man has labored, amid the ever-recurring changes of form, to recognize the invariability of natural laws, and has thus by the force of mind gradually subdued a great portion of the physical world to his dominion (1864:I, 1–2).
>
> Nature considered *rationally* . . . is a unity in diversity of *phenomena* . . . the most important result of a rational inquiry into nature is, therefore, to establish the unity and harmony of this stupendous mass of force and matter, to determine with impartial justice what is due to the discoveries of the past and to those of the present, and to analyze the individual paths of natural *phenomena* without succumbing beneath the weight of the whole (1864:I, 2–3; emphases ours).

He stresses the balance that should be kept between the study of detail and that of the whole:

> . . . in the sphere of natural investigation as in poetry and painting the delineation of that which appeals most strongly to the imagination, derives its collective interest from the vivid truthfulness with which the individual features are portrayed (1864:I, 13).

Humboldt, perhaps through his connections with Ritter or perhaps through his personal investigations, also considers culture in relation to the environment. Talking of the tropics and their uniformly stable environment, he wonders why these regions rarely gave rise to high civilization:

> . . . but as far as tradition and history guide us, we do not find that any application was made of the advantages presented by these favored regions. Recent researches have rendered it very doubtful whether the primitive seat of Hindoo civilization—one of the most remarkable phases in the progress of mankind—was actually within the Tropics (1864:I, 14).

Proceeding then to an examination of the cold zones:

> . . . it is to the inhabitants of a small section of the temperate zone, that the rest of mankind owe the earliest revelation of an intimate and rational acquaintance with the forces governing the physical world. Moreover, it is from the same zone . . . that the germs of civilization have been carried to the regions of the Tropics . . . (1864:I, 15).

One may compare Ritter's view that the rise of civilization is always centered along river valleys (Nile, Euphrates, Huang Ho, and so on), and mainly along the lower river valleys where rich bottom lands are available.

Humboldt has the following to say of scientific method:

> Physical philosophy . . . doubts because it seeks to investigate, distinguishes between that which is certain and that which is merely probable, and strives incessantly to perfect theory by extending the circle of observation . . . it is the special object of the present work to combat those errors which derive their source from a vicious empiricism and from imperfect induction (1864:I, 17–18).

Humboldt is concerned with laws, though his laws certainly are not simple and unilinear affairs. They are intricately interwoven and depend upon each other:

> In proportion as laws admit of more general application, and as sciences mutually enrich each other, and by their extension become connected together in more numerous and more intimate relations, the development of general truths may be given with conciseness devoid of superficiality. On being first examined all phenomena appear to be isolated, and it is only by the result of a multiplicity of observations, combined by reason that we are able to trace the mutual relations existing between them (1864:I, 29).
>
> It is not the purpose of this essay on the physical history of the world to reduce all sensible phenomena to a small number of abstract principles, based on reason only. The physical history of the universe . . . does not pretend to arise to the perilous abstractions of a purely rational science of nature. . . . All points relating to the accidental individualities, and the essential variations of the actual . . . (1864:I, 29–30).
>
> An equal appreciation of all branches of the mathematical, physical and natural sciences, is a special requirement of the present age . . . (1864:I, 33).

The physical description of the world he calls physical geography, which he defines and describes as follows:

> The ultimate aim of physical geography is, however, as we have already said, to recognise unity in the vast diversity of phenomena, and by the exercise of thought and the combination of observations, to discern the constancy of phenomena in the midst of apparent changes. In the exposition of the terrestrial portion of the Cosmos, it will occasionally be necessary to descend to very special facts; but this will only be in order to recall the connection existing between the actual distribution of organic beings over the globe, and the laws of the ideal classification by natural families, analogy of internal organization, and progressive evolution (1864:I, 43).

It is possible that Boas was also influenced by Alexander's brother, Wilhelm v. Humboldt, who, as Basilius (1952) has documented, stated many of the ideas about language and culture which anthropologists today ordinarily associate with the names of Sapir, Whorf, and Dorothy Lee. A number of scholars have pointed out that the "Sapir-Whorf hypothesis" was stated—though less clearly and in more qualified form—by Boas and published by him several times and as early as in his (1911) introduction to the *Handbook of American Languages* (cf. Hoijer 1952:92–3). However, we have not encountered in Boas' writings any reference to the work of Wilhelm v. Humboldt on this issue.

*Karl Ritter* (1779–1859). Humboldt and Ritter were acquainted with each

other and maintained correspondence (Bruhns 1873:II, 213 and passim). But while Ritter brought all of his enormous knowledge to bear upon the relationship between man and his physical environment, and while in that inquiry he remained essentially a student of empirically investigated phenomena within that realm, Humboldt had both broader interests encompassing all the natural sciences and a considerably more philosophical approach. As Fischer was narrower than Ritter, so Ritter was narrower than Humboldt.

Ritter's importance as a predecessor of Boas is suggested in the following statement:

> . . . dass er nach den das Anthropogeographische hervorhebenden Historikern des vorigen Jahrhunderts wieder der erste ist, der in wissenschaftlichen Darstellungen der Geographie den Beziehungen zwischen Boden und Geschichte nachgeht und im Nachweis derselben den höchsten Zweck der Erdkunde sieht (Wünsche 1899:3).

In the earlier literature geography was conceived of as a purely descriptive subject with no attempt to investigate natural-historical causes, but Ritter argues, for example, that the reasons for Europe's importance in the geographical-historical setting of his own times is due to certain natural conditions (Naturverhältnisse):

> Seine Stellung in der Mitte der durchschnittenen Landhalbkugel; sien überaus günstiges Verhältnis von Küstengliederung; mitten inne stehend zwischen Armut an Gliedern einerseits und Zersplitterung andererseits; eine gewisse Harmonie seiner plastischen Gestaltung und Klimatik im Vergleich zu der Einförmigkeit oder den grellen Kontrasten anderer Erdteile, und damit eine gewisse Überschaulichkeit aller seiner Raumverhältnisse. Dadurch war ein Schauplatz geschaffen, auf dem sich Völkerorganisationen herausbilden konnten, die diejenigen aller anderen Erdteile weit übertrafen (Wünsche 1899:4–5).

According to Ritter's anthropogeographical researches, the effects of nature and geography on man as a "racial" being and on man's actions are not clearly separated. He believes that both are modified and conditioned by geographical and climatological conditions, and others, in roughly equal measure.

The term "history" as used by Ritter denotes a variety of phenomena which broadly comprise all those which deal with the problems of primitive peoples as well as those of a purely historical nature. Thus in his treatises he covers questions of migrations, of small tribal groups as well as of entire tribes, conquests, colonization, peaceful infiltration, traffic, transportation, diffusion of religions, and other ideas.

Nowhere, however, does Ritter give a theoretical outline of his ideas and methods. These can only be gathered from occasional statements in the introductions to some of his books and studies, or from the chapters of his monumental 19-volume study, *Erdkunde* (1817 and subsequent years). Matters that seem to have interested Ritter especially are:

1. The tendency of nomadic peoples when put into major migratory motion to "infect" adjacent nomadic tribes by their own movements, i.e. to set them, too, on the move.

2. The absence of an absolute geographical deterrent on any given continent to the movements of peoples.
3. The tendency of mountain ranges to deflect population movements.
4. The question of varying intensity of population movements.
5. The problem of geographically preconditioned routes of population movements or, what Ritter calls, the "Law of Migrations."

Ritter believes that most migratory movements start among nomadic peoples. The steppe character of their environment makes for mobility, and to this he adds the tendency toward "good" tribal organization among them. Climatologically and geographically milder regions adjacent to the unfavorable nomadic habitats attract the nomads. However, the ultimate cause is population pressure. Poor steppe country cannot carry an increased population, and tribes spill into adjacent regions. Such movements cause counter-movements on the part of the attacked sedentary peoples in the environmentally and geographically more favorable neighboring regions. These counter-movements result in a gradual decrease of the territories inhabited by the nomads. Implied here, of course, is the intrinsic superiority of the sedentary peoples; this is the more interesting, since Ritter believes the nomads to be more courageous than the intrinsically cowardly sedentary peoples:

Neben der Fruchtbarkeit des chinesischen Reiches war es die Feigheit der Chinesen welche die Mongolen und Mandschuren immer wieder zu Einfällen reizte (*Erdkunde*, IV, 728).

In sum, Ritter views the nomads always as attackers and the sedentary peoples always as defenders who, by clever exploitation of the environment, tend to increase their geographical territories. Though Ritter believes that no geographical feature can ultimately arrest the movements of peoples, he believes that mountains are strong deflectors of such movements. As an example:

An dem natürlichen Bollwerke der Tauruskette scheiterten im 7. Jahrhundert alle Eroberungsversuche der Araber. Der von Süden kommende Völkerstrom brach sich hier und wandte sich nach Osten, wo das Land offen stand. So wurden Mesopotamien und Persien erobert, und von dort aus, in der Richtung von Osten nach Westen, drang dann der Islam erst in Kleinasien ein (*Erdkunde*, XVII, 604).

Ritter views population movements as being continuous and occasionally catastrophic events. He opposes the historical school which postulates such violent movements as followed by static conditions:

Nordiran zeigt eine unaufhörliche Völkerbewegung, die nicht nur in einem Hin- und Herziehen der Nomaden zwischen Sommer- und Winter-stationen besteht, sondern sich zu Fehdezeiten nach ganz verschiedenen Richtungen und Gebieten ausdehnt. . . . Es ist eine stetig fortschreitende Völkerwanderung (*Erdkunde*, VIII, 373).

Or:

Im 7. Jahrhundert dehnten die Araber ihre Eroberungen bis zum Oxus und zum Atlas und Ebro aus; Arabien glich in jenem Jahrhundert einer ununterbrochen fliessenden Menschenquelle (*Erdkunde*, II, 111).

Since Ritter views "historical" movements as continuous, they begin for him already in prehistoric times. This line of reasoning led Ritter to the conclusion that the primitive peoples of his day were living merely on a prehistoric level, i.e. he uses the introduction of writing as the dividing line between history and prehistory, since only through writing can events be recorded and thus become history (*Erdkunde*, IV, 276).

Ritter was aware of the value of linguistics in anthropogeographical research. Discussing population movements in Africa he states:

> Keine Spur von Kunstdenkmalen als Urkunden einer einstigen höheren Völkerthätigkeit, ist in diesen übereinander und durcheinander gewachsenen Völkerstämmen aufbewahrt. Nur die vielfach verschiedenen Spuren der Sprachstämme leiten hie und da auf ihre Succession [hin] (*Erdkunde*, I, 417).

His analyses led Ritter to derive a "law of migrations" of peoples. The basic observations underlying this "law" rest upon the fact that many important historical movements have been in an east-west direction. Where observable, such movements were always singularly favored by geographic conditions. On the whole, however, he believes that his "migration law" applies only to primitive peoples and not to members of "higher" civilizations. He frequently contradicts his own "law of migrations" by proving exactly the reverse. An interesting example is his observation that:

> . . . die amerikanischen Völker auch schon in ihren Urzeiten, genötigt von geographischem Bedingungen, nordsüdlich gerichtete Wanderungen ausgeführt haben (Wünsche 1899: 87).

He also attempted to formulate a "law of culture growth." It runs thus:

> Der Entwicklungsgang der Völker schreitet in einer von der Natur vorgeschriebenen Bahn fort, die immer von den Höhen zu den Tiefen, von den Quellen der Flüsse zu den Mündungen geht (paraphrased from *Erdkunde*, X, 286).

The emphasis here is on rivers; down-river the valleys broaden; fertile land abounds and thus conditions are favorable for the development of "civilization." In the uplands, however, there are only poorly irrigated flat-lands; these are the habitat of the nomads. In justice to Ritter, it should be mentioned that he admitted many exceptions also to this "law."

In summary, it may be stated that Ritter was in many respects an environmental determinist. His method was empirical; he used deductive reasoning in his speculations on prehistory and the nature of primitive contemporary societies. History is for Ritter a continuum. There is some evidence of his awareness of the usefulness of linguistics in determining migrations on a "primitive" level. Ritter was interested in formulating laws with regard to population movements and geography as well as to culture development and geography. His data on peoples are derived from historical as well as from other sources such as reports of missionaries, travellers, and so on. His studies are therefore both synchronic and diachronic. He has no notable views on race

except for statements correlating "laziness" of certain races with climatic conditions. The line between "race" and "peoples" is never clearly drawn. The climatic effect on man is mainly in terms of temperament. Thus, when talking of Africa, Ritter states:

> Das Glut-Clima, das sie bewohnen . . . regt ihr Gefühle zur grössten Leidenschaftlichkeit, ihre Phantasie zu den grössten Missgeburten auf, macht sie roh und unbändig (*Erdkunde*, I, 409–410).

In another quotation, again with reference to Africa, he seems to imply that skin color (as well as "man's entire nature") is subject to the tropical environment:

> Wie schon die Hautfarbe des Afrikaners minder indifferenziert ist als die aller anderen Menschen, nicht weiss, sondern noch dem dunkel färbenden Prinzip unterworfen, so auch ist seine ganze Natur wie die seines Erdteils den physischen Mächten der Tropenwelt mehr ergeben (*Erdkunde*, I, 415).

Ritter has a poor opinion of mixed bloods, which he shares with many contemporary writers:

> Als Grenzvölker zwischen Arabern und Negern als wahre Mulatten, haben sie [the Moors] von beiden die Laster in sich aufgenommen, ohne ihre Tugenden zu besitzen (*Erdkunde*, I, 410).

*Theobald Fischer* (1846–1910). Both in method and content, his work closely follows that of Ritter, but he was much more restricted in his knowledge and ambitions and largely confined his research to the Mediterranean Basin. On the other hand, Fischer did studies similar in pattern to some of Ritter's: papers on the date palm (1881, expressly dedicated to Ritter) and the "oil-tree," olea, (1904) which have their exact counterpart in Ritter's study of cotton (1851) and its distribution. Fischer closely follows Ritter in viewing culture development in terms of all aspects of geography. Fischer is less concerned with population movements and less deterministic. His theoretical stand can easily be gathered from this quotation:

> Wie uns ein volles wissenschaftliches Verständnis der Oberflächenformen des Festen nur dadurch erschlossen wird, dass wir dieselben nicht mehr als etwas schlechthin Gegebenes lediglich beschreiben, sondern als etwas Gewordenes und in steter Weiterentwicklung begriffenes unter steter Bezugnahme auf die geologischen, tektonischen, petrographischen, klimatischen Verhältnisse usw. ursächlich erklären, so muss auch das gleiche Verfahren auf die Betrachtung der Umrisse des Festen, auf die Gestaltung der Küsten angewendet werden. Denn nur so treten uns die Beziehungen der Länder zum Meere, die Bedeutung der Küsten für Verkehr und Kulturentwicklung, für eigentümlichen Erscheinungen im Völkerleben, für die Verteilung und wechselnden Geschicke der menschlichen Wohnplätze klar entgegen, nur so können wir die Frage, ob ein Küstengebiet und sein Hinterland die Bedeutung, welche es heute hat—von der Einwirkung menschlicher und geschichtlicher Verhältnisse abgesehen—stets ge abt hat oder stets haben wird (1885; reprinted 1908:59).

Like Ritter, Fischer is concerned with formulations of "laws" though he does not actually formulate them in such an explicit manner:

> Derartige Untersuchungen sind im letzten Jahrzehnt häufiger angestellt und unsere Erkenntnis geographischer Gesetze dadurch ausserordentlich gefördert worden (1885; reprinted 1908:59).

In his study of the date-palm Fischer not only discusses the distribution of this economic plant but also discusses the customs associated with its use among diverse peoples. The cultural significance of the date-palm is summed up in a number of conclusions which the author believes can be drawn from his geographical, ethnological, ethnographical and distributional studies:

(a) Not only do dates feed millions of peoples but the presence of the date-palm actually predetermines the cultural and economic usefulness of many localities (such as the date-oases along the caravan routes in Arabia). The distribution of the date-palm has thus increased the traversibility and inhabitability of huge desert stretches.

(b) However, the date-palm and its date crop do not permit man to rise to a significantly high culture level, because they need little care.
Sie zwingt ihn [den Menschen] daher nicht zu zusammenhängender Thätigkeit und zu völlig sesshaftem Leben, sie erhebt ihn höchstens auf die Stufe des Halbnomaden und lässt ihn Jahrtausende hindurch in denselben Lebensgewohnheiten, denselben Sitten und Anschauungen, denselben Bedürfnissen verharren. Darum finden wir in den weiten Gebieten ihrer Verbreitung überall dieselben Kulturzustände, dieselbe Lebensweise, mögen sich auch noch so verschieden geartete Stämme der Dattelcultur widmen (1880:85).

(c) Dasselbe Verharren auf derselben Stufe materieller wie geistiger Kultur oder wenigstens sehr langsames, kaum merkliches Fortschreiten, und selbst dieselbe Art der Gottesverehrung, welche ihrem ganzen Wesen nach so recht für dasselbe geeignet, in dem ganzen Gebiet so rasch durchgedrungen ist und allein herrscht, möchte ich als einen Ausdruck der Gleichförmigkeit und der Schranken betrachten, welche die Natur in der Zone, die sie mit der Dattelpalme beglückte, zugleich dem menschlichen Geiste gezogen hat (1880:85).

Fischer's writings are eminently readable, since he largely dispenses with the well-known German scientific jargon sometimes known as "Gelehrtendeutsch." He is always interesting, and his papers are packed with facts. Occasionally Fischer ventures into the realm of race and ethnic groups. He seems less gifted than Humboldt and Ritter, but we speculate that he communicated to his pupil a passionate concern for detail and an enthusiasm for his subject.

*Bastian* (1826–1905). Bastian has been treated by Lowie (1937) and in other familiar anthropological sources. Lowie makes most of the points that are relevant here. Bastian was even more cautious than Virchow regarding Darwinian evolutionism, displaying a "puritanical empiricism." He stressed "the danger of system-mongering and the dependence of psychology on ethnology." He "clamored for the harvesting of facts." He demanded proof of assumed historical connections. His "geographical provinces" seem to anticipate "culture areas." All of these things are surely reminiscent of Boas.

We should like to add a little. Bastian believed that there are no racial types which determine culture (1870:VIII), and he also rejects language as a classificatory criterion because language is a phenomenon:

> . . . über deren Vielfachheit der Erscheinungen auf der Erde uns noch jedes Urtheil fehlt . . . und zwischen deren vermeintlich typischen Formen nirgends fest markirende Striche gezogen werden können, da die Grenzen . . . vielfach ineinander überlaufen (1870:VIII).

Of great interest, however, is his emphasis upon the value of linguistics because it gives us:

> . . . einen tiefen Einblick in historische Beziehungen sowohl wie in das Walten psychologischer Gesetze (1870:VIII).

One wonders if Bastian was not a primary source of Boas' interest in linguistics, though Boas wrote Lowie late in life that his treatment of language was based on Steinthal (Lowie 1943b). Steinthal was the only linguist whom Boas met in his student years, ". . . but Boas was not yet interested in language and afterwards he regretted never to have attended the lectures of that enquiring thinker" (Jakobson 1944:188). Nevertheless, we find Boas studying the Eskimo language intensively before he went on his first field trip. Possibly this was a stimulus from Bastian, who appears to have been one of the first scholars to concern himself seriously with languages outside the Indo-European and Semitic families. Possibly—or in addition—Boas had informally acquired more knowledge of and interest in linguistics during his student years than he recollected later. A history of Bonn University refers to the Bonn of Boas' period as a place of "Hochblüte" of linguistics (Bezold 1920:512).

Some of Bastian's statements on environment could have come out of Ritter or Fischer. Thus he speaks of a tribe ". . . als Ausdruck seiner geographischen Umgebung" (1870:XIV). Bastian, like Boas, was a holist:

> . . . die Ethnologie . . . hat sodann die Gesammtentwicklung eines Volkes in der ganzen Fülle seiner Phänemene zu überschauen, die Productionen seiner Psyche, die zwar auf dem Körper, also auch auf dem Schädel basirt, aber nicht davon allein abhängt—seiner Psyche, die zwar in der Sprache ihren Ausdruck findet, die aber sich selbst die Sprache . . . schafft (1870:IX).

The suggestion of relation between language and "mentality" again recalls certain of Boas' utterances.

Of course, Boas did not follow Bastian altogether on the Elementargedanken, and yet we find him as late as 1938 writing:

> In many cases a clear enunciation of the elementary idea gives it the psychological reason for its existence. . . . The dynamic forces that mold social life are the same now as those that molded life thousands of years ago (1940:194–95).

In a selection revised for publication in his volume of papers, he says:

> I do believe in the existence of analogous psychical processes among all races wherever analogous social conditions prevail; but I do not believe that ethnic phenomena are simply expressions of these psychological laws (1940:316–7).

*Influences on Boas.* From Humboldt, Ritter, Fischer, and Bastian came a preoccupation with actual phenomena in all their detail; a concept of diverse and multiple causes; an interest in the environment; a preoccupation with the interrelations of history, peoples, and their environments; and a strong sense of the complexity of the historical process. Specific influence from Ritter is apparent in Boas' paper "Über die ehemalige Verbreitung der Eskimos" (1883:120, 124), written before his first field trip.

So far as theory is concerned, Humboldt and Boas seem much the closest. There are in Boas' writings many echoes of Humboldt's favorite phrases: "ever-recurring changes of form"; "vivid truthfulness with which the individual features are portrayed"; "the individual paths of natural phenomena"; "distinguishes between that which is certain and that which is merely probable, and strives incessantly . . . by extending the circle of observations"; "only by the result of a multiplicity of observations"; "it is not the purpose . . . to reduce all sensible phenomena to a small number of abstract principles." Compare just a few statements from Boas (the first four published by Boas in 1887 and quoted in Buettner-Janusch 1957):

> It is only since the development of the evolutional theory that it became clear that the object of study is the individual, not abstractions from the individual under observation.
>
> Our objection to Mason's idea is that classification is not explanation.
>
> . . . the object of our science is to understand the phenomena called ethnological and anthropological in the widest sense of those words—in their historical development and geographical distribution and in their physiological and psychological foundations. These two branches are opposed to each other in the same way as are biology and the so-called systematic "organology" . . .
>
> In ethnology all is individuality.
>
> The analysis of the phenomena is our prime object (1940:268; originally published in 1930).
>
> An error of modern anthropology, as I see it, lies in the overemphasis on historical reconstruction, the importance of which should not be minimized, as against a penetrating study of the individual under the stress of the culture in which he lives (ibid. 269).[4]

On the other hand, Boas did not want to perfect theory by reason, as Humboldt did, nor did he share Humboldt's doubts of "vicious empiricism." From men like Fechner and Virchow came influences which placed Boas squarely in the tradition of radical empiricism. On the question of holism, of Humboldt's conception of "die Natur als ein durch innere Kräfte bewegtes und belebtes Ganzes aufzufassen," we judge that Boas was ambivalent. He swings back and forth between "embracive" and "isolative" tendencies.

## THE "ISOLATIVE" APPROACH

*Gustav Fechner* (1801–1887). Fechner was quite aware that all discourse proceeds from premises:

> . . . jede irrige oder mangelhafte Voraussetzung . . . durch den Einfluss den sie

auf unser Denken, Fühlen und Handeln gewinnt . . . [zieht] Nachteile nach sich und verkehrte Handlungen (1919:II, 91).

He also knew that conviction in the recurrence of regularities in empirically observed phenomena rests, in the last analysis, on "faith":

Die ganze Naturwissenschaft geht auf Erzeugung solchen Wissens, obschon das so erzeugte Wissen in letzter Instanz immer an dem Glauben hängt, dass die Verallgemeinerung, auf der wir fussen, und die daraus gezogenen Folgerungen ferner ihre Bestätigung finden werden, wie sie solche bisher im Kreise des Erfahrungmässigen gefunden haben. Denn weder durch Logik noch Erfahrung lässt sich beweisen, dass solche eintreten müssen (1910:5).

Only the combination of theory and empirical observation will lead to laws which:

. . . keine theoretische Folgerung zu gestatten, die praktischen Forderungen widerstrebte, wie aber auch umgekehrt die Vereinbarkeit der praktischen Forderungen mit thoretischen Folgerungen zu verlangen (1910:XVII).

Nevertheless, there is a constant insistence on the search for abstract generalizations through induction:

Verallgemeinerung durch Induktion und Analogie und vernünftige Kombination des von verschiedenen Seiten her gewonnenen Allgemeinen sind meines Erachtens die einzigen thoretischen Wege und Weisen, die uns im Gebiet der geistigen wie materiellen Wirklichkeit zu in sich haltbaren und für die Erfahrung wieder fruchtbaren Grundlagen des Wissens über das Selbstverständliche und unmittelbar gegebene hinausführen können (1919:I, XV).

Although Boas criticized Fechner (Boas 1882: 575–6), it is evident that he took Fechner as his exemplar during his "psychophysical period." In a letter to his parents from Berlin on December 11, 1882, Boas speaks proudly of the fact that Fechner has cited him in a new book. And we suspect that the stress upon the "gegebene" and the presupposition of discoverable regularities and causality which enter so frequently into Boas' earlier papers in anthropology derive as much from Fechner as from any single source. We may note, in passing, that Fechner was the only academic psychologist who influenced Freud (Jones 1957:268).

*Rudolf Virchow* (1821–1902) was a physician, pathologist, and a liberal political leader. He also published 1180 titles in anthropology, 523 of them in physical anthropology. There was a connection through Dr. Jacobi (husband of the younger sister of Boas' mother) who had been one of Virchow's students, as had Bastian.

There are many parallels between Boas and Virchow. Both had interests and knowledge of immense range. Both were tremendous empiricists and intense workers. Both had exceptionally high standards as to what constitutes "proof." Both were political activists, and tenaciously fought prejudice and chauvinism. The motto attributed to Pericles, "Prosperity lies in freedom alone, and freedom lies only in courage," was equally that of Virchow and

Boas. Virchow was the first eminent 19th-century German to argue for the intrinsic equality of races and his publications on this subject caused an uproar in the Second Reich (cf. de Grousilliers 1880).

A number of features of Boas' scientific thinking may be traced to Virchow. We may instance his relative lack of interest in Darwinian evolution and his scepticism about Mendelian heredity. Dr. Cecil Yampolsky remembers conversations with his father-in-law in which their doubts of Mendel's theory were discussed and in which they agreed that LaMarck was still to be reckoned with. Boas occasionally makes guarded references to biological evolution in his writings, but his students report that he did not discuss it in his seminars. The only citation to Darwin we have discovered is one to *The Voyage of the Beagle*. Kroeber (1943:7) observes that Boas turned his attention to demonstrating that some human traits are not transmitted according to simple Mendelian rules. Reserve on both these matters probably emanated from Virchow. It is not true, as has been alleged, that Virchow "rejected" Darwinism but rather he objected ". . . that only established facts and results, and not theories, should be taught in the schools" (Jacobi 1881:23).

Ackerknecht has called attention to two other influences from Virchow:

> . . . it seems significant in view of Boas' later work on changes of cranial indices of descendants of immigrants, that he had been exposed to Virchow, perhaps the only man in his generation to uphold the plasticity of races and the possibilities of change in cranial indices. . . . How strongly Boas was influenced by Virchow in this field is shown by his paper on the relative merits of the length-breadth and length-height indices of the skull (1953:235).
>
> One cannot help feeling that Boas' arguments against social evolutionism are to a large extent transcription of Virchow's polemics against Haeckel in the question of biological transformism (1953:236).

Virchow was even more empiricist than Fechner. He felt strongly that "observation and experiments only have a permanent value" (Jacobi 1881:11). Boas once said to a graduate student "there are two kinds of people: those who have to have general conceptions into which to fit the facts; those who find the facts sufficient. I belong to the latter category." It should be noted, however, that a different viewpoint could be documented by selective citation from Boas' publications.

Many of Virchow's most basic standpoints emerge clearly from the following extracts from his rectoral address to the University of Berlin (1893:24-7):

> Man begriff eben mehr und mehr, dass die Naturwissenschaft nur in der Beschäftigung mit der Natur selbst erfasst werden könne und dass zu einer dauernden Verbindung der Wissenschaft mit den realen Dingen grosse Anstalten erforderlich sind: Museen, Sammlungen, Laboratorien, Institute. Ganz besonders trat dies hervor, als erkannt wurde, dass der Versuch das wichtigste Mittel ist, die Natur zu einer Antwort über das Wesen, die Ursachen und das Geschehen eines Vorganges zu zwingen. Aus den Studierzimmern der Philosophen war kein Aufschluss über wirkliche Naturvorgänge hervorgegangen. Seitdem der Glaube an Zauberformeln in die äussersten Kreise

des Volkes zurückgedrängt war, fanden auch die Formeln der Naturphilosophen wenig Vertrauen mehr.

Jetzt verlangt man von dem Gelehrten, dass er auch ein Forscher sei, und die Ansprüche in Bezug auf die Lehre haben sich so sehr gesteigert, dass schon der akademische Unterricht sich die Aufgabe stellt, die lernende Jugend nicht blos in die Methoden, sondern auch in die Praxis der Untersuchung einzuführen. Es bedarf keiner besonderen Beweisführung mehr, dass diese Art der Wissenschaft eine nützliche sei.

Vielleicht sind die sogenannten exakten Naturwissenschaften, Physik und Chemie, auf lange Zeit gesichert, seitdem an die Stelle der vielen Naturkräfte die Erkenntnis von Ihrer Einheit getreten ist. Aber jene grosse Schaar von Disciplinen, welche das Leben und seine Geheimnisse zu ihrem eigentlichen Vorwurfe gemacht haben, die gesamte Biologie, ist noch keineswegs gesichert und gerade von da aus hat von jeher der Mysticismus seine schlimmsten Einbrüche vollführt.

Unsere Zeit, die in ihrem wissenschaftlichen Gefühl so sicher und siegesfroh ist, übersieht eben so leicht, wie die frühere, die Stärke der mystischen Regungen, welche von einzelnen Abenteurern in die Volksseele getragen werden. Noch steht sie ratlos vor dem Rätsel des Antisemitismus, von dem niemand weiss, was er eigentlich in dieser Zeit der Rechtsgleichheit will, und der trotzdem, vielleicht auch deshalb, fascinierend selbst auf die gebildete Jugend wirkt.

What Boas (1902:443) wrote in his obituary of Virchow describes equally well Boas' own fundamental creed:

. . . his position rests on the general scientific principle that it is dangerous to classify data that are imperfectly known under the point of view of general theories, and that the sound progress of science requires of us to be clear at every moment, what elements in the system of science are hypothetical and what are the limits of that knowledge which is obtained by exact observation. To this principle Virchow has adhered steadfastly and rigidly, so much so that many an impetuous student has felt his quiet and cautious criticism as an obstacle to progress. On this account he has suffered many hostile attacks—until generally the progress of research showed that the cautious master was right in rejecting the far-reaching conclusion based on imperfect evidence. There are but few students who possess that cold enthusiasm for truth that enables them to be always clearly conscious of the sharp line between attractive theory and the observation that has been secured by hard and earnest work.

To an extreme degree Boas and Virchow followed Santayana's maxim: "scepticism is the chastity of the intellect, and one should not surrender it too easily to the first comer." One must not, however, consider that Virchow did more than provide an intensely admired example of views that Boas had developed before he met the great man. At the age of 18 Boas wrote (in letters to his mother and sister, respectively):

Es ist zu dumm, dass man gar kein Urtheil über ein solches Werk hat, weil man ja gar nicht wissen kann ob die Thatsachen alle richtig sind . . .

Das sehe ich jedenfalls jetzt ein: Wenn einer durch eine einzelne Thatsache irgend etwas Allgemeines beweisen will, so hat er gewöhnlich unrecht, denn es lassen sich gewöhnlich eine ganze Menge anderer anführen, welch das Gegentheil beweisen . . . .

Nevertheless it remains significant that Boas felt close personally, as well as

intellectually, to Virchow. Mrs. Yampolsky has informed us of this, and speaks of Virchow's warmth when she and her father called on him in his Berlin home.

*Summary.* Virchow was at least as interested in detail as any of the four men in our "embracive" group, and was certainly more antitheoretical than Humboldt. He did not seek "laws" as avidly as did Fechner and others, but he shares with them the disposition to study isolated aspects of phenomena rather than the phenomena in their holistic concreteness. Both tendencies are manifested in varying proportion in Boas' works (cf. Swartz 1958), and Boas himself (1940:311) stated that he did not consider the two approaches "mutually exclusive."

From the totality of the influences upon him, Boas appears as a 19th-century scientist in one very important sense. As Buettner-Janusch (1957:321) says:

> The concept that scientific laws are found in nature rather than formulated by the scientist is typical of Boas. It is a view that has few adherents in modern science.

In the 19th century, science—whether it consisted in the depictive integration of phenomena or in the formulation of abstract regularities—was "discovery." That is, the "reality" was there, and the task of the scientist was only to find it and order it as it existed, ordered, in the world of nature. The 20th-century scientist regards himself more as a "disciplined inventor." That is, he creates models or constructs from experience which he does not necessarily regard as faithful portraits of nature. Schlick has put it that science is not a picture of the real world but a symbolic system by which our experiences can be correlated to each other in a practical way.

After a careful re-reading of Boas' collected papers (1940) we are convinced of one thing: he started with enthusiasm for the discovery of laws of cultural development—as might have been expected of one with his training and background—but was reluctantly forced to a different position. Compare two early statements made with two of a generation later:

> 1888: A comparison of the social life of different peoples proves that the foundations of their cultural development are remarkably uniform. It follows from this that there are laws to which this development is subject. Their discovery is the second, perhaps the more important aim of our science (1940:633–4).
>
> 1896: . . . certain laws exist which govern the growth of human culture, and it is our endeavor to discover these laws (1940:276).
>
> 1930: The complexity of cultural development is so great, and the conditions that determine the course of historical happenings are logically so entirely unrelated, that the attempt to give an adequate explanation of the history of any individual society in regard to biological type, language and culture seems hopeless (1930:109).
>
> 1932: Cultural phenomena are of such complexity that it seems to me doubtful whether valid cultural laws can be found. The causal conditions of cultural happenings lie always in the interaction between individual and society, and no classificatory study of societies will solve this problem. The morphological classification of societies may call to our attention many problems. It will not solve them (1940:257).

However, the note of ambivalence is struck both in the early and later periods. The difference rests in the slanting toward one pole or the other. In his famous 1887 paper,[5] "The Study of Geography," which Boas—perhaps significantly—placed at the very end of *Race, Language, and Culture*, he contrasts Comte's view of the sciences which "have the one aim, to deduce laws from phenomena," with Humboldt's *Cosmos* where every phenomenon is considered "as worthy of being studied for its own sake." "Its mere existence entitles it to a full share of our attention; and the knowledge of its existence in space and time fully satisfies the student, without regard to the laws which it corroborates or which may be deduced from it" (1940:642). This is a favorite theme with Boas: "But a single action or event is interesting, not because it is explainable but because it is true" (1940:644). Conversely, in his later period he is not quite willing to abandon "laws" altogether. In his 1932 presidential address to the AAAS, for example, he says "It is true that some valid interrelations between general aspects of cultural life may be found . . . " (1940: 257). In his article on "Anthropology" in the *Encyclopedia of the Social Sciences* he refers to "a few of the phenomena that may be expressed in the form of generalizations" and remarks that "General laws may also be discovered referring to specific relations between various aspects of social life" (1930:110).

Wax (1956:67) well notes: " . . . the cosmographer values geography as a study of individual phenomena for their own sake, much as does the artist," and he quotes from Boas (again from "The Study of Geography"):

> The origin of every science we find in two different desires of the human mind—its aesthetic wants, and its interest in the individual phenomenon. . . . The more clearly all phenomena are arranged, the better will the aesthetic desire be satisfied, and for that reason the most general laws and ideas are considered the most valuable results of science.

This links beautifully with Kroeber's suggestion (in the same issue of the American Anthropologist) that a neglected clue to the understanding of divergent features of Boas' thought is the presence in his scientific work of strong but somewhat over-controlled or suppressed esthetic trends. There is something of the artiste manqué. At any rate, we agree with Kroeber when he writes:

> In linguistics, accordingly, Boas seems to me to have satisfied two sets of impulses at once: one was a quasi-aesthetic, form-centered, recognitional impulse of which he was only partly aware. This he largely re-channeled into his second and dominant intellectual and analytic set of impulses (1956:159).

All this fits with a number of known facts:[6] Boas' warm and lively responses to literature during his gymnasium period; his at least partial identification with the embracive, phenomenological trend; the esthetic activities in his personal life; his professional interest in the arts; the "artistic" bent of some of his best known students. But in his exploration of the interstices and limits of experience, he imposed upon himself a severe set of constraints.

NOTES

[1] We thank Helene Boas Yampolsky, Cecil Yampolsky, Gene Weltfish, Alexander Lesser, M. J. Herskovits, and Gertrude Hess (assistant librarian of the American Philosophical Society) for substantial help in the preparation of this paper.

[2] Sources of which we have discovered English translations we quote in that language; sources available only in German are quoted in the original.

[3] Information on Boas' university teachers was obtained largely from various volumes of the *Allgemeine deutsche Biographie* 1875–1912; Leipzig: Duncker und Humblot, 56 vols.

[4] Cf. Boas' Forewords to Mead's *Coming of Age in Samoa* (1928) and to Benedict's *Patterns of Culture* (1934).

[5] A letter written shortly after this paper was published indicates that he worked exceptionally hard to arrive at this statement of his position and that he regarded the position as in some sense definitive.

[6] After reading these pages in manuscript, Weltfish comments: "I gathered the notion from him that intellecting was never a grim exercise on any level and that creativity was firmly based on enjoyment."

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# *Apprenticeship Under Boas*

MARGARET MEAD

THE myths that obscure the personality of an intellectual leader gather thickest in the years immediately following his death, when there are many people alive who speak with varyingly authoritative voices, and the next younger generation listens. As one of Boas' students, and a student of his later, mellower years, I feel that it will be useful to include in this memoir something of the effects, as I see them, of the way he taught and the way he appeared to those who were learning from him in day-to-day contact.* Melville Herskovits (1953) has written of Boas, using extensive documentation from his published work and fusing his account with his own long-time association with Boas, as a student and a research worker. I shall not attempt to duplicate this, but shall confine myself to some aspects of Boas' relationships to his students and to his role in anthropology as it appeared to me as a student—a subject to which Herskovits devoted only a couple of pages.

At the period (1922–1929) during which I was successively an undergraduate student, a graduate student, a research student, and a student assistant, Boas was playing several different roles in American anthropology. He saw the whole of the study of man—as he had once seen the life of the Central Eskimo (Boas 1884)—spread out in a great panorama, stretching back to the infrahuman world and forward as man gained greater control over those aspects of culture which he saw as cumulative—technological and scientific development. We were never permitted to forget man's oneness with the rest of the living world, or the problems that arose when man became the first domesticated animal. Within this great panorama Boas saw the scientific task as one of progressive probing into a problem now of language, now of physical type, now of art style—each a deep, sudden, intensive stab at some strategic point into an enormous untapped and unknown mass of information which we would some day master. No probe must go too far lest it lead to premature generalization—a development which he feared like the plague and against which he continually warned us.

* This paper is based on my own experience and notes taken during the period 1922–1925, when I was a student in Boas' courses at Columbia; on Ruth Benedict's notes for the period 1921–1925; on an examination of the Boas letters, now in the library of the American Philosophical Society in Philadelphia, and of correspondence with Ruth Benedict, now in the files of the Benedict Collection in Vassar College Library. One version of these materials has been published as part of *An Anthropologist at Work: Writings of Ruth Benedict* (Mead 1959). I have also consulted Herskovits' book on Boas (Herskovits 1953), and the series of papers which appeared in the American Anthropologist between 1953 and 1958. The present paper is designed primarily to assist younger anthropologists, anthropologists in other countries, and anthropologists who never knew Boas, in understanding controversies about him and the reasons for the various sorts of assignments of importance and negative and positive influence that have been made in recent years.

Secondly, he himself, as a curious and wondering human being (and he never lost that quality of wonder which seems to be one of the essentials of greatness), was enormously preoccupied with what we today would call microcultural problems—problems of cultural change and development where individual differences and slight historical circumstances are magnified into long-term, significant effects. So, in seminars in which students reported on current pieces of fieldwork, he drew our attention again and again to the small, vivid detail and its possible relevance within the larger framework.

Thirdly, he had cast himself in the role of one of the responsible leaders of a giant rescue operation to preserve the vanishing fragments of primitive languages and cultures. This had to be done with almost no money, very few trained people, and no time to spare. His letters are filled with discussions of how some small sum—two or three hundred dollars—was somehow to be raised to keep someone writing up material or to keep someone a couple of months longer in the field. While he was extraordinarily exacting in the standards which he set for himself and his students, he also welcomed—because this was necessary for the sake of salvage—any concrete reliable work from anywhere. Once welcomed, and then almost immediately used, the data had also to be rigorously criticized. So, in addition to the criticism which he meted out to those who made premature and inadequately documented generalizations, there was criticism of the methods that lay back of the fragments with which he, and we, had to work.

Fourth, he thought of himself as morally responsible for the uses made of anthropology, for the provision of research materials relevant to human freedom, and also for keeping the subject itself open-ended. This sense of moral responsibility manifested itself in his reviews, his criticisms of Museum policy, his plans for great, inclusive expeditions and institutions, and what often seemed his high-handed interference in other people's plans. As he was not working for personal power but for the good of mankind, he had the moral freedom that goes with self-elected, complete dedication. It is true that he did not found a school, because he did not believe in schools. Yet he was in fact a prophet of an open-ended, continually revised scientific enterprise, and for this very reason he stood in opposition to all prophets, to all exclusive and final solutions of any problem,[1] and had little compunction in attempting to stop others from becoming prophets. So he wrote to Tozzer,[2] objecting to the possibility of a large research grant for anthropological research being put under the direction of Radcliffe-Brown, since that would mean only one approach to a question that required many. Dillydallying, low-level politics, academic red-tape—all threw him into a rage. I remember his description of a dream he had after a research council planning meeting—a dream in which he was trying to hang a curtain on a curtain rod that was made of rats biting each others' backs.

This sense of moral responsibility made us all feel that we were caught in an obligation to do more and better work, or we would "betray Dr. Boas."[3] A mandate as vague as this carried with it its own heavy sense of responsi-

bility. The extent to which Boas left us on our own was appropriate in a period when letters to and from the field might take several months, but it also produced that peculiar sense of obligation that accompanies the ill-defined task. I remember walking the floor of my office in the American Museum the day I was to appear to have my manuscript of *Coming of Age in Samoa* (Mead 1928a) criticized, saying over and over again, "I suppose I have betrayed him like all the rest"—in the sense of having failed to make the most of a unique chance, of funds to do fieldwork when funds were so few, and of my "field" when fields were every day being spoiled for ever.

The form that these very preoccupations took when he taught us was a curious amalgam, with consequences which have outlived him. He never talked about science or scientific method as such, except as he discussed mathmatical methods in his formidable Anthropometry course which all of us had to take. He would take a theoretical finding and destroy its validity by citing chapter and verse around the world that showed it wasn't so. When it came to the study of culture, there was no discussion of the law of parsimony or of controls or of sampling, no invocation of models, none of the paraphernalia of the natural sciences with which students of human behavior who were more interested in being "scientists" than in understanding human behavior were beginning to surround themselves. We did not doubt that we were doing scientific work, that we had to live up to scientific standards and use all the methods, qualitative or quantitative, that were or would become available. But we did not talk about methods; instead, we talked about problems, the problems that should be tackled next.

Characteristically, there are no methods named after Boas, just as there is no Boas school, and when we went into the field, we made such methodological innovations as seemed called for by the problem and the local setting. We made these innovations almost unconsciously, or, as Boas would have said, "automatically." In Samoa I decided to include three complete communities so that I would have a total sample, and I set up cross-cutting age groups for study, chose special intelligence tests, and used the deviant individuals to delineate the pattern—without any sense that these were "methods," which would later be named and dignified with italics and capital letters, e. g., the study of the community in vivo, the cross-sectional as opposed to the longitudinal study of child development, the attempt to provide "culture-free tests," and so forth. With our attention focused on problems and with standards of work built in, we exemplified the kind of extension of the teacher's skill which is characteristic of apprenticeship learning, where the learning is inexplicit, rather than more formal kinds of teaching where learning is acquired through textbooks and formal demonstrations. It is significant that Boas disliked textbooks and seldom sent us to his own written work. He gave us the gist of it in his lectures and assumed that we would resort to it only when we needed the actual material.

It is probably also important that he did not teach us by laying out a systematic plan for future research, such as is possible in an experimental

science. The outcome, in terms of science, would still depend upon what was actually found in ethnography, in archeology, in paleontology, in natural history. Some problems would never be solved, but the next field trip might shed unhoped-for light on some problem that no one had yet glimpsed. This recognition of the continuous possibilities of illumination from the material became as conscious as the idea of methods as such remained unconscious. Ruth Benedict defined her approach to the contrast between Plains and Pueblo cultures not as a method of analysis but as resulting from a sudden insight (Mead 1959, Part III); and even such an obvious point as my recognition that it was nonsense to study adolescents without also studying preadolescents, I felt as an insight, not as methodology.

This inarticulateness about method has perhaps been a disadvantage. Students trained in other universities have learned only to recognize a method if it is labelled as such, and a good many of the innovations we made went unrecognized and had to be reinvented. In an article about Ruth Benedict (Barnouw 1949:252) Victor Barnouw remarked about Boas:

> When Franz Boas published page after page of blueberry-pie recipes in Kwakiutl, the old man probably knew what he was after; but when his students did the same kind of thing, they often lacked the driving central purpose which animated Boas.

It was not until I puzzled irritably about the meaning of this comment that I verbalized to myself just what Boas had been doing: that is, building an exhaustive corpus of materials which could be analyzed for negative as well as for positive points. And the materials which he chose—tales, recipes, technologies, art objects—were all of a kind that permitted the collection of an exhaustive corpus relatively independent of the idiosyncrasies of the individual collector.

Similarly, I realized the implications of sampling in anthropological field method only when I had to discuss problems of sampling with social psychologists and sociologists, as for example, where the sociologists argued that the 100 families included in the sample for a family study should have no contact with one another, while the anthropologist argued in favor of going from one family to another. What became clear was that, in a closed and well-studied community, every item of information on one individual is also an item of information on every other individual in the group (Mead 1955). So, too, Boas' dicta in his letter to Mason (Buettner-Janusch 1957:319)—but stated less definitely in the classroom—lay back of our development of the appropriate unit for the study of food habits (National Research Council 1945). But in 1945 I saw our way of working in this study as a consequence of ethnological field responsibilities rather than as a formal, methodological point. Not until I tried to formulate an appropriate unit for the study of cultural evolution for the Simpson and Roe symposium (Mead 1958), did I realize that this unit was in fact the one that I had been using in the field for thirty years, i.e., the small, interacting human group in which each individual member is specified.[4]

This lack of self-consciousness about method could even lead to the making

of a kind of somnambulistic choice. In 1953, we realized that one of the crucial changes which had taken place in Manus culture since the first study in 1928 was in the attitude toward boundaries in time and space. This showed up clearly in the way children filled a piece of drawing paper, and I complained, "If only I'd tested them on paper of different shapes in 1928!" But later, when I again went over the sets of drawings collected in 1928, I found that I had done this, and so could demonstrate the change that had taken place by 1953. Automatic methodological behavior of this kind is only transmissible from person to person. So, for example, it is striking that even with the degree of explicitness that has been introduced into the use of still photography as a research tool in anthropology, our method has been used only by those who have had research contact with Gregory Bateson or myself, and discussions of the method turn into attacks on the approach.[5]

Boas called his course on theory "Methods." The distinction between covert and overt culture is now treated as a theoretical point, but when I first discussed this problem in 1933, I included it—together with a discussion of event-triggered as compared with calendrically-triggered cultures—in an article called "More Comprehensive Field Methods," so continuing the tradition (Mead 1933).

Here I have drawn on my own experience; elsewhere I have documented the development of Ruth Benedict's anthropological approach. Another student of Boas after whom no methods have been named is Melville Herskovits, although he has pioneered a whole field of Afro-American research. Likewise, an examination of the methods used by Ruth Bunzel in her study, *The Pueblo Potter* (Bunzel 1929), would provide another set of innovations: the use of papier maché models, the introduction of pots from other areas which the artist was asked to copy, a self-apprenticeship with analysis of Zuni teaching methods. Here method was so merged with problem that the methodological —and so to a degree the scientific and generalizable as opposed to the unique and historical contribution to the study of art—was blurred.

Ruth Bunzel described the circumstances in this way:[6]

> You want to know how I came to write *The Pueblo Potter* and became an anthropologist. It is really a story about Boas and the things he believed in. It began in the spring of 1924. I had been working for Professor Boas for two years as secretary and editorial assistant and was becoming increasingly involved with anthropology, but without any formal training whatsoever. That summer Boas was going to Europe and Ruth Benedict was going to Zuni to collect mythology. I thought that if I could see an anthropologist at work at the most crucial and mysterious part of his study, and perhaps try a bit on my own, I would know whether or not I wanted to be and could be an anthropologist. So I thought that I would take my vacation time and a few dollars I had saved for a trip to Europe and instead meet Ruth Benedict in Zuni. My plan was not too ambitious—I was a good stenographer and I would take down folk tales and interviews in shorthand, and do all our typing. Ruth Benedict seemed pleased with the suggestion so I took it to Boas.
>
> Boas heard me out, snorted in his inimitable fashion and said, "Why do you want to waste your time typing?" (He always thought typing a "waste of time," though

Heaven knows he wasted enough of his own precious time on similar donkey work to know that the gremlins didn't do it after funds were spent on informants.) "Why don't you work on a problem of your own?" I said that I didn't think I was equipped to do a "problem of my own," but he paid me no mind and went on. "You are interested in art. Why don't you do a problem in art? I have always wanted someone to work on the relation of the artist to his work." (Boas always had a long list of problems that he hoped someone would work on, things that he had started and hadn't had time to go on with, or things that had just occurred to him.) I asked how one would go about investigating such a problem; he recommended that I look at a big manuscript on Salish basketry by Teit and Haeberlin (Haeberlin et al. 1925); I might get suggestions from that.

This plan raised something of a tempest in our little teapot. Elsie Parsons was outraged; sending an untrained person into the field, and to a pueblo at that! She threatened to withdraw her support of the mythology project were I permitted to go. Boas stuck to his point. Intelligence and will were what counted. Granted enough of these one would find the necessary tools. (You know how he always refused to give reading lists, hated examinations, scorned erudition for its own sake. One learned what one needed when one needed it. It was the sense of problem that was important. He always advised students to spend less time reading and more time thinking.) Elsie Parsons capitulated; I was permitted to go.

It was the end of the academic year. Boas went off to Europe; Elsie Parsons went to the Caribbean; Ruth Benedict went to New Hampshire. I stayed in New York with my "problem"—and the basketry paper. The basketry paper proved to be a study of variation. It had nothing to do with what I conceived my problem to be although it contained many points on such matters as the influence of technique on design and analysis of style that stood me in good stead later. I was really alone in a big sea and I had to swim. I assumed that Zuni artists were not going to be any more articulate about what they were trying to do than the poets and painters I had met in Greenwich Village, and that direct questioning would get me nowhere. I had to approach the problem indirectly. I decided on three lines of approach—criticism, instruction, and problem solving. I gathered together as much relevant photographic material from the American Museum of Natural History and the National Museum as I could lay my hands on. I got together a lot of drawing materials. But one thing that I had learned from the Salish basketry paper was that painting a design on paper was not the same problem as putting it on a pot. So I made *papier maché* pots to take along for my informants to paint designs on. Three weeks later I was on the train to Zuni. After five weeks in Zuni I had found out what I wanted to know there, and moved on to other pueblos—this was not part of the original plan, but it was what made the Zuni material tell.

I was too ignorant at the time to know that I was pioneering; that I was on the frontier of a whole new field of anthropology; that this was the first tentative approach to the study of the individual in culture; the first attempt at a systematic study of behavior. I didn't know that I was employing "participant observation" and "projective techniques" because I had never heard of these things. *The Pueblo Potter* does not have a chapter on methodology.

*The Pueblo Potter* was written the following winter, on weekends between my job. When I showed the first draft to Boas he said, "It seems to cover everything," and I was satisfied. I decided to become an anthropologist, and I learned about "methods." The following summer I went back to Zuni to learn the language and prepare to find

out what it felt like to be a Zuni; I saw this as an extension of the study of the artist. Elsie Parsons sponsored this trip. A gallant lady, she acknowledged in her own gracious fashion that Boas had been right.

A consideration of the advantages and disadvantages of this kind of dependence upon face-to-face contact between teacher and student who are working at the growing edge of a science is too extensive a topic for discussion here. But when accusations of antiscientism are hurled at the man who made anthropology into a science, some knowledge of just how deeply and inexplicitly Boas ground his methodological approach into our bones should help students to understand him.

As further illustrations of some of the points made in this article, I have included a few selected materials from my own notes, from Ruth Benedict's notes, and from Boas' own letters. These are designed not to prove but to exemplify the points I have tried to make.

My notes on Boas' first lecture on Methods, for the spring term in 1925, read:

Development of civilization may:

1. Follow general laws

or

2. Unique historical developments which have to be studied where they occur. Most fundamental question is whether there is anything common to mankind the world over and at all times.

1. Man everywhere has articulate language—language with grammatical organization
2. Everywhere uses tools (not meant any kind of artificial object) but use of things shaped intentionally for particular purpose
3. (Fire)
4. We always find a certain kind of aesthetic principle—a certain valuation of form—
    1. in ethics
    2. in aesthetics

    Idea of valuation is one which is common to all groups of mankind.
5. Actions adapted to ends become the subject of rationalization giving a reason why things are done.

The development of these [first two] characteristics is essentially a pre-human development and none of these features is exclusively human.

He then went on to discuss those categories among birds and animals, the rare cases of tool using, and the "strictly analogous" building of "shelter," the presence of "articulate communication among animals but no grammatically organized system," the decoration of nests, "but we have no clear indication that any idea of values really occurs to animals," and mentioned that "reason is often read into mechanical reproduction of learned adaptive activities." He then suggested that a point for the beginning of the study of man would be where all four of these behaviors were present.

In Boas' discussions of method, his comments would be like these:

In a lecture on parallelisms (February 17, 1925) in which he discussed polyandry among the Todas and the Eskimo, he said, according to my notes:

In reality the two ethnic phenomena with which we are dealing are not the same. So our task is not to emphasize the similarities but to go into detail with the dissimilarities.

In lecturing on Graebner the following week (February 24, 1925), he discussed the area of diffusion covered by the "Earthdiver" tale and the parallel diffusion, at a much later date, of bead embroidery, and commented:

So we find various elements of cultural life are gradually being distributed over the continent, but in entirely different ways. Psychological-socio conditions which bring about the distribution of a tale and a technique are quite different.

Later, in a seminar in which Ruth Benedict reported on "folklore" (April 23, 1925), Boas mused at the back of the room and to no one in particular:

So many of the widely distributed stories enter into the most sacred tales of the people. This is difficult to understand. *There must be a particular reason why they should be selected.* Earthdiver story for example (emphasis supplied).

And the student registered again, but quite inarticulately, an expectation that one day the diverse cultural phenomena would be ordered.

In a lecture on Cassirer (March 17, 1925), he said:

*If we had enough information,* I believe we would find the relation of time is clearer than space—that the idea of sacred time as against secular time does occur over and over again. . . . *But the proof that could be given is not easy. It would be necessary to have all the material the world over.* Justifiable query to ask: Are these three relations [space, time, and number, as Cassirer gives them] borne out the world over? If they are we might say with Cassirer: We do have these fundamental characteristics of human thought.

We have to be very critical and skeptical. Have these universal ideas developed historically or are they determined psychologically? *And if these ideas are not universal but scattered, unhistorically,* there might be psychological determinism under certain given conditions (emphasis supplied).

The teaching in the Department of Anthropology, all of which Boas did himself—for this was a period when anthropology at Columbia was still suffering from the effects of Boas' unpopularity during World War I—consisted of the series of lectures on anthropological theory (which he called "Methods"), a seminar in which he participated as a member and occasionally reported on a piece of work in which he was interested, a series of fast-moving, condensed lectures on anthropometry, and very detailed exercises in and explorations of the grammar of various languages—sometimes using texts which he had collected, sometimes using a native informant. The whole structure was held together by undertones and cross-references which still remain strangely vivid. When I was editing *An Anthropologist at Work* (Mead 1959), I made a list of the points in Methods which I believed had had the most influence on Ruth Benedict and then resigned myself to the task of finding the lectures in question, either in Ruth Benedict's notes or in my own. However, I found that she

herself had selected and put together in an envelope the very set of lectures for which I was searching.

I am reproducing selections from her notes here. For purposes of clarity the words which she had abbreviated have been spelled out in full. I have preserved the pattern of words on the page, the punctuation, and her own additions in parentheses as she made them, in order to give a more vivid picture of how the points Boas made appeared to her as she heard them for the first time. This is of course also the form to which she returned in later years, where someone who had not been Boas' student would have turned instead to *The Mind of Primitive Man*, to Boas' words unmediated by the form, handwriting, and personal selection. Not all pages were dated and new pages are indicated by asterisks.

## Ruth Benedict's Notes on Boas' "Methods" Lectures Spring 1922[7]

She had chosen the February 7 lecture on the study of similarities within dissimilarities.

February 7 1922
. . . Fundamental thing we want to understand is their imaginative treatment of their everyday experiences. What are their everyday experiences? e.g. Imaginings in regard to wish fulfillments—overcoming nature etc. True in *all* material, i.e. different *psychological* sources for same developments? in behavior-moieties. Botanical analogy—very far-reaching morphological similarities, *but* in different families (Old & New World). If I stress this point, it is not because similarities are not important. Quite the contrary. Just as it is important to know the morphological similarities in Old & New World plants. But two problems: 1. the fact of similarity (to be established & the similarities especially within the dissimilarities). Then follow both back psychologically.
(Caution for actual material.)
How can we go about it when we want to study the basis of the similarities—
(Keep in mind: may be different)
2. How far can historical depth be determined?
One constant consideration: Transmission *never* a self-contained cultural life—even Bushmen and Esquimo who are intensely hostile to neighbors.

The next selection, five weeks later, dealt with the mechanisms of transmission within a culture.

March 16 1922?
What makes for cultural stability or change?
Conservative Elements—Example
Does not need elaboration, for are ever present.
Example: Pagan elements in European Christianity
Vierkandt—stetigkeit
G. Tarde—Les Lois de l'imitation
As long as we ask "Why does society repeat last generation's—?"—obscurity. Rather "the individual"—"why repeat actions of preceding generation?"
His upbringing in the family—
Unconscious and unintentional imitation plays very large part

People assuming dialects of people they are among—even contrary to will
Pace of people in N. Y., and small city
Imitation *very* deep seated—
basis of personality—begins before child speaks, etc. "Open the door" doesn't know what *door* means. Imitated as a whole, because has certain kind of effect.
Ought to lay great stress on this tendency toward imitation in life of child. Habits (become) are by their nature dogmatic, very hard to break.
Use of knife by African (=gravers tool, *toward* him)
and Indian
Mexican Indian and SW Indian, trot with carrying head-band
China over shoulder, very different
Arrow release—Morse
Arrow between 1 & 2nd finger China
Arrow between thumb & doubled fingers Europe
Yes & no gestures—cultural
South American raise circle of nose for no—wrinkle forehead by raising "eyebrow"
All these imitations of such a character as become automatic. Are not voluntary.
Disgust ordinarily accompanies divergence from own habits.
Smacking the lips in table manners Kwakiutl
Accompanied by very intense emotional resistance, but seems to my mind only a secondary consequence of the automatic character of these actions.
* Literal meanings not necessarily present to speakers.
a *technical* analysis. English—"understand" (penknife)
Algonkian—fall down=movement, to come to an end, on the ground(?)
Kwakiutl—steamboat=moving in the water with fire on its back.
Do the metaphors shape the thought of speakers? (N.B. Note that total number of stems limited, in every language. Yet number of expressions *are* built metaphorically.)
Can't say. Sometimes mechanical, sometimes not. Not easy to determine. Can see it (perhaps only) in poetry—and see that their imagery is determined by linguistic form.
2 questions: Relation of grammatical form to thought form. Has been attempted in European languages. Would be important too in primitive languages. Relation of metaphorical expression to thought form. No material collected. Would lead to important results.
Diffusion—
"In attempting to reconstruct general cultural history, the tendency to consider each feature due to inner forces. The realization of transmission leads us to entirely different results."
Stringed instruments (not discovered by each experimenter)
Wheat
Tales ( . . . can take place only where conditions are such that one person would tell tale to *amuse* person of other language and other people.) Note also: the original much greater diversity of languages. Cannot see that linguistic barriers in any way hinder the distribution of tales. . . .

The March 20 lecture contains the only reference which I ever heard Boas make to Freud, in lecturing, and also foreshadows contemporary studies of nativistic cults.

March 20 1922
Break of automatic habit very difficult when . . . conflict develops.

Freudian unjustifiable to my mind to extend causality to this extent. Action actually depends on number of causes logically unrelated. No inner relation to stimulus to which he is exposed.

To exclude accident from mental life does not seem admissible.

Example—*some* forgetting due to causes he describes; but *all*?

??especially weather—is determined but subject to accident for can't see nexus.

Vitiates whole Freudian introduction: that the full explanation of behavior is to be found in these preceding experiences.

The artificial behavior of children determined socially

The imposed and the imitative behavior of the young are fundamental sources of conservative tendencies of society

Formal education imposes definite ideals

Taboos in primitive society

Technique, chipping stone, basketry, trade groups

Behavior of the crowd

Of course we know that individual does not react in same way alone or with others, as he reacts one way when excited—Parade—peculiar kind of emotional disturbance in bystanders as well as— . . .

Different influences of crowd phase—

1. To *repeat* cultural pattern—North West Coast ceremonial
2. To *disrupt* cultural pattern—Shakers of Washington, Salvation Army . . . —not by any means always conservative.

The March 23 and 25 lectures which she had selected also deal with problems of internal change.

March 23, 1922

Observations of influence of group upon behavior of child in primitive tribes almost impossible to get. Must be observations among ourselves—much more (on individual behavior) investigations than commonly—

Local developments without any sort of external influence

Polynesia—example: Samoa, Hawaii, Easter Island

Eskimo—certain regions where any differences *must* be due to inner development.

cv Africa

Labrador and Greenland Eskimo—700 years no external influences. Examples following small, but doesn't matter if characteristic.

Kayak—not Smith Sound—might be geographical for so much ice, dangerous

Clothes—Baffinland, long tail for women, Arctic

Greenland, short tail . . . Distinct also for N & W Coast of Hudson Bay

Mythology—interest shifts

Easter Island, Gilbert Islands, Marquesas

In favorable areas may determine what occurs from inner forces without external influence

Also study of local details in any area (cf. zoological study of endemic forms) making due allowance for outer influences.

Also archaeological means.

Summary: Hard to obtain material to prove inner development but what we do know sufficient to show that it is always to be reckoned with.

Linguistic—Phonetic shifts—pervading whole language

Similar in all parts of world

Similar to biological changes which may also parallel each other in isolated communities in different parts of the world.

Assume too often—Example: where inner structure (inanimate things) unaffected by history, i.e. stone in one place or another, Example: where iron magnetized

History will determine its structure

History cannot be undone; certain effects will always remain which will influence future activities.

Actions of society depend on accumulated experience of individuals . . .

March 25 1922

Inner Changes

Drift in certain definite directions

Orthogenetic idea present in much theoretic discussion—Tylor's animism, Haddon's Art, Ehrenreich's Mythology—Have to examine critically in how far such a steady drift is actually present in ethnic phenomena:

Example: linguistic, i.e. phonetic drift.

*Counting

Terms of relationship

Ours: generation; sex of person spoken of. . . .

Indian: Relative age, Reciprocals, Opposite and like sex

What does this show in regard to Levy-Brühl's position

1. Primitive classifications do not agree with ours. Example: Constellations—Picture of Dipper and knowledge that are six stars exist side by side in civilized mind

Others have picture of whole starry universe as a whole—but may also have concept of individual stars

Daylight and whole aspect of early morning world separated from sun

Dream image and living man "identified?" But, from certain point of view, belong to same category.

Modern also

According to Preuss—primitive man's always wrong, ours always right.

Modern Fictions treatment of fever, etc.

Difference is that because of organized experience more of ours falls into realm of causality cf. Modern personification—How much reality given to it?—to creative mind, much at time of creation.

Differs.

So primitive

Poetic view of world ever so much more powerful—and leads to action. Each individual participates to very much greater extent in artistic life of people than in modern—based on certain unbounded (to us) sorts of classifications. But does not prove that primitive man cannot think logically and clearly.

Cf. modern, same 2 sorts of thinking.

Durkheim on primitive man. Crowd psychology

Opportunities for crowd action in primitive life very numerous. Anything that is established will have a chance to make itself felt very strongly.

The last lecture chosen emphasized the importance of both kinds of thought, the poetic and mystical as well as the rational and scientific.

May 10 1922

. . .

Human mind operates to very large extent with traditional experience, and importance of mystic element in mental make-up varies with individual. World may be viewed

from teleological or causal point of view, and this mysticism may enter in both, more likely in former. No society where both not present. In *both* cases the logical and the a-logical. When do not understand the causal relations, *both* act a-logically. Accident is so irregular ordinary mind does not conceive it as belonging to causal realm, and consequently, amulets, etc. As scientific knowledge grows the chance for accident lessens.

Example: tested tree for canoe on North West Coast proves rotted at core. We ("best educated") still say "Just my luck." Scientist recognizes working of the law of probabilities. Is a question of *knowledge*, not mental attitude.

In the seminars, Boas moved back and forth in the discussions, now considering large general principles and now wondering, with the delighted fresh curiosity of a child, about some small point. In a seminar in which Thelma Adamson reported on Secret Societies in North America and West Africa (December 10, 1924), my notes record his comment:

It's interesting to follow out the question of principles of social grouping and function—kinship, age class, election—I think we'd get similar distribution of function—there may be certain elements that predominate—most interesting problem to compare the principles of organization and function.

Or in a seminar in a series in which the effects of density of population were being considered and in which Eleanor Phelps (Hunt) reported on Aztec and East African societies (February 4, 1925), he said:

Next question is historical development of these types. Material so meagre, so defective, still—area like Europe—definite tendencies develop with complexity—like increase in size of unit. In America, Mexico and Peru—against whole American background. I presume we'd recognize certain peculiar lines of development due to complexity. Types a reference back to an earlier statement that "we can speak of a number of different types which develop in historical areas" must be taken as descriptive types—inductive —not total number of possibilities.

In the seminar (February 25, 1925) in which Ruth Bunzel reported on her work later published in *The Pueblo Potter*, she had put a series of examples on the blackboard. My notes record the following interchange:

Boas: In all the cases on the blackboard difference in number of units on band and body.

Bunzel: No relation. The Zuni will always tell you 4 or 5 is best, 3 and 6 good, 5 and more than 6 never used, predominantly arrangement is 3.

MM: Wouldn't tendency not to have mirror design militate against optimum 4? (No answer)

Boas: This independence of bands is also true in Tlingit basketry. (Draws a basket design on the board.) Two of these independent of the rest. In the basketry of the Chilkat three independent bands. *How far is this true of banded designs in the world?* In Mexican pottery painting becomes the background. This is true of designs in prehistoric Europe.

Here, in this discussion, the scientist is speaking—the interest in detail wholly within a context of possible generalization and ultimately of abstraction and law. Later in the same seminar, and only nine months after Ruth Bunzel had entered anthropology, Boas addressed her as a colleague:

What is your idea of the theory that geometrical forms must go back to basketry? The step design, particularly in combination with the curvilinear is hard otherwise to account for. A relationship between the conditioned design and free paint design doesn't seem unlikely.

The minimum direction that Boas gave his students has been described trenchantly by Herskovits (1953:23):

Dissertation topics were brought to Boas for his approval, not suggested by him. Methods employed in obtaining and handling data, and the determination of the manner in which they were to be cast in final form, were the business of the student, to be worked out independently by him; only results were presented for appraisal.

From the wealth of wonder and speculation buried in the on-going process of the seminar and the lectures on Methods, we selected our topics. My experience was typical. I decided that I would like to work on the relative stability of different elements of culture, a topic to which Boas' comments periodically returned. I asked for an appointment and stated briefly—there was every encouragement to be brief in talking to someone who never seemed to have any time for dalliance with living—what I wanted to do. His advice took perhaps three minutes:

You could do this in Siberia, but that would mean knowing Russian and Chinese. The Low Countries might be a good place but that would mean a lot of work with old Flemish, medieval Latin, etc. There is not good enough material to use the Algonkian-speaking peoples. You might try Polynesia where you would only need French and German.

The interview was over. I presented one seminar report and had one conference after the thesis (Mead 1928b) had been completed, in which Boas suggested that I expand my comments on Graebner in the introduction.

When the time came for my first fieldwork, the discussions again followed a characteristic course. I wanted to go to Polynesia, an area in the literature of which I was now at home; Boas disapproved of my going so far away into the unhealthful tropics. I wanted to study the relative affective strength of new and old elements in culture; Boas, always tailoring a particular piece of research to the exigencies of theoretical priorities, time, place, and personal ability, wanted me to study adolescence. In the end we compromised. I took his problem and he consented to my choice of Polynesia, providing I would work on an island to which a boat made regular visits. This accounted for the fortunate choice (as it turned out) of Samoa rather than one of the other Polynesian islands. In the single interview which we had, he cautioned me against being drawn into conventional ethnological enquiries and warned me that I must be prepared to waste a great deal of time just sitting about getting to know the adolescents. Later, just before I left, he wote me a letter (July 14, 1925) in which he gave me the only other explicit advice I received.[8] It begins characteristically:

I am sure you have thought over the question very carefully, but there are one or two points which I have in mind and to which I would like to call your attention, even if you have thought of them before.

One question that interests me very much is how the young girls react to the re-

straints of custom. We find very often among ourselves during the period of adolescence a strong rebellious spirit that may be expressed in sullenness or in sudden outbursts. In other individuals there is a weak submission which is accompanied, however, by a suppressed rebellion that may make itself felt in peculiar ways, perhaps in a desire for solitude which is really an expression of desire for freedom, or otherwise in forced participation in social affairs in order to drown mental troubles. I am not at all clear in my mind in how far similar conditions may occur in primitive society and in how far the desire for independence may be simply due to our modern conditions and to a more strongly developed individualism.

Another point in which I am interested is the excessive bashfulness of girls in primitive society. I do not know whether you will find it there. It is characteristic of Indian girls of most tribes, and often not only in their relations to outsiders, but frequently within the narrow circle of the family. They are often afraid to talk and are very retiring before older people.

Another interesting problem is that of crushes among girls. For the older ones you might give special attention to the occurrence of romantic love, which is not by any means absent as far as I have been able to observe, and which, of course, appears most strongly where the parents or society impose marriages which the girls may not want . . .

. . . Stick to individual and pattern, problems like Ruth Bunzel on art in Pueblos and Haeberlin on Northwest Coast. I believe you have read Malinowski's[9] paper in Psyche on the behavior of individuals in the family in New Guinea. I think he is much too influenced by Freudianism, but the problem he had in mind is one of those which I have in mind. . . .

The urgency of the disappearing primitive cultures and of the cruel inequalities in the world on which research was grievously needed was communicated to us not by preachments but by tempo, tone, and gesture, and this urgency remained with us.

Nor did Boas ever lose it himself. In 1930, at the age of 72, he made a return trip to the Kwakiutl to try out a new method—the use of film—on problems of style. From Fort Rupert, on November 9, 1930, he wrote to Ruth Benedict:[10]

I talk with difficulty and understand after I write it. I follow conversation only partly. It goes too rapidly but I am getting into it again.

From Beaver Harbor, on November 13, he wrote:

The dance problem is difficult. I hope the films will give us adequate material for making a real study. In music there are a number of quite distinct styles: summer songs, morning songs, love songs, but I shall not unravel that problem while here. The language also has baffling problems. There is no *good* informant because all are satisfied with a variety of forms. I rather think this is due to a merging of several dialects into one which is not equal in all individuals. I am mindful that this is an acculturation problem and I hope we shall get enough on that point.

And later, on November 26, he wrote again:

I am worrying now about the style of oratory because I do not yet know how to get it down. Anyway I have my troubles with ordinary conversation. Narrative I can understand quite well, if they talk distinctly, but many have the Indian habit of slurring over the ends of their words—whispering—and that makes it difficult.

These letters I showed to an especially skillful handwriting analyst, and her first impression was that it was the writing of a quite young man, uncertain of himself!

## NOTES

[1] Konrad Lorenz has suggested that the hallmark of genius is the interpretation of a great mass of phenomena according to a single explanatory principle, which will then become, in later generations, a "special case," for example, Pavlovian conditioning or the Freudian Oedipus Complex. Therefore, Lorenz does not regard Darwin as a genius. Certainly Boas exemplifies the negative image of single-minded commitment to single solutions of problems. (Unpublished lecture by Konrad Lorenz on: Physiological and Psychological Aspects of Motor Patterns: Afferent Spontaneity and Relations to Learning, delivered as part of Second International Seminar on Mental Health, Postgraduate Center for Psychotherapy, New York, October 26, 1958.)

[2] Letter from Franz Boas to Alfred Tozzer, December 2, 1932. Boas Archives, American Philosophical Society, Philadelphia.

[3] Letter from Ruth Benedict to Margaret Mead, from Pena Blanca, August 29, 1925. (Mead 1959:294.)

[4] "It is only since the development of the evolutionary theory that it becomes clear that the object of study is the individual, not abstractions from the individual under observation." (Boas 1887; quoted in Buettner-Janusch 1957:319.)

[5] Cf. the omission of any reference to *Balinese Character: A Photographic Analysis* (G. Bateson and M. Mead 1942), in the article on Technical Aids in Anthropology: A Historical Survey, by John Howland Rowe in Kroeber 1953 (895–940). Dr. Rowe explained the omission as due to his disagreement with the approach of the book (Tax 1953:202–205). The work on still photography has been carried on by Frances Macgregor, Theodore Schwartz, Leonora Shargo Schwartz, and Ken Heyman, in the use of films in the work of the American Museum of Natural History Admiralty Island Expedition, 1952–1954, and Balinese Expedition, 1957–1958; and a much finer step in intensive film analysis is involved in the use of Gregory Bateson's recent films in the forthcoming *Natural History of an Interview*, co-authored by Bateson, Birdwhistel, Brosin, Hockett, and McQuown (McQuown ed.).

[6] Personal communication from Ruth Bunzel, 1959.

[7] These notes, in this form, and the original manuscript notes on which they are based, are in the long draft of *An Anthropologist at Work*, and in the Benedict Lecture Notes File of the Benedict Archives, Vassar College Library.

[8] Letter from Franz Boas to Margaret Mead; carbon in Boas Archives, American Philosophical Society, Philadelphia.

[9] These three references are to: Bunzel 1929; Haeberlin et al. 1925; and an article by Malinowski, originally published in the English journal called Psyche, and later republished (Malinowski 1927).

[10] Letters from Franz Boas to Ruth Benedict. Benedict Archives, Vassar College Library.

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# *Boas' "Natural History" Approach to Field Method*

MARIAN W. SMITH

IN 1938, looking back on his life, Boas wrote: "An early intense interest in nature and a burning desire to see everything that I heard or read about dominated my youth" (Boas 1938:19). He goes on to speak of his "intensive, emotional interest in the phenomena of the world" (1938:20), and it is this active, almost devotional, attitude to "the surrounding world" which gave his fieldwork its distinctive character.

Born in the same year which saw publication of the *Origin of Species,* Boas could profit from the first great intellectual response which greeted man's entrance into the world of nature. The preceding years had seen the foundation in France, Britain, and the United States of anthropological societies formed to encourage the "study of man in all his varieties" (Constitution, American Ethnological Society, 1842) and the prevailing, forward-looking view was incorporated in the title of a book published in London in 1850: *The Natural History of the Varieties of Man* (Latham 1850). For the first time since the days of Athens, mankind was the legitimate subject of the kind of research which could be given to the other orders of creation. The museums of natural history included man and his products in their exhibits of the earth's wonders, extending their coverage to men of many times and many areas, but the universities had not yet caught up with the new surge of interest and the youthful Boas had to turn to geography as a formal study. He says, in contrast, that it was on account of his "intellectual interest" (Boas 1938:20) that he studied mathematics and physics. It has been pointed out many times that Boas' later work in anthropology combined his training in the diverse disciplines of geography and physics, but perhaps his unique contribution lay in the direction of his interest in natural phenomena—and it is that side of his work on which this paper will concentrate.

Boas more than any other person first brought the very mind of man into the natural world. As early as 1902 he recognized that the philosopher himself adopts much of the current thought of his environment (Boas 1902:872–74). Conceptualization and philosophy no longer breathed a finer air. They could be studied by the same techniques and approached by the same attitudes as other human characteristics, and consequently lost much of their aura of revealed truth. Man was now to be studied not only in all his varieties, but in all his activities.

The complete emergence of man into the natural world could only be achieved if men everywhere were equally deserving of study. The doctrine of social evolution which isolated certain cultural phenomena as superior to others effectually prohibited adequate classification. Behavior must be studied in its own terms, classified according to its own features. The thought processes of the investigator must therefore be subjected to similarly rigid examina-

tion. Toward the end of his life, Boas wrote ". . . I fought 'the old speculative theories' as I am now fighting the new speculative theories based on the imposition of categories derived from our culture upon foreign cultures. . . " (Boas 1940:311, from a section that was apparently added to the original text of "History and Science in Anthropology: A Reply" when the paper was reprinted.) In one of his final papers, he returned to this methodological problem and there placed it in direct relation to the problem of classification (Boas 1943:314).

The continuity in Boas' thought which I have here chosen for emphasis is thus marked by two aspects: a distrust of traditional ideas, and faith in the natural world. He was fully conscious of both, and cites them in that retrospect of his early life to which reference has already been made. Having been "spared the struggle against religious dogma that besets the lives of so many young people," he suffered two shocks which he regarded as of "permanent influence" on his life:

> As I remember it now, my first shock came when one of my student friends, a theologian, declared his belief in the authority of tradition and his conviction that he had not the right to doubt what the past had transmitted to us. The shock that this outright abandonment of the freedom of thought gave me is one of the unforgettable moments of my life. I had been taught in home and school that it is our duty to think out our problems according to the best of our ability. The denial of this duty, or rather the claim that what I considered a duty was a wrong, seemed unbelievable to me. A second shock was a series of conversations with an artistically gifted elder sister to whom my materialistic world seemed unendurable (Boas 1938:19–20).

Many people share a distrust of traditional ideas and authority, and faith in the natural world. But the implications of these two attitudes, which Boas spent so much effort in developing, have not yet been fully realized. The position which Boas assumed may be conveniently characterized as the natural history approach to the social sciences. Because this position seems to me to be the foundation of Boas' ethnographic method, I shall elaborate it in contrast to another, equally prevalent among social scientists, which can be referred to as the social philosophy approach. In the United States, anthropology and sociology typically represent these two approaches. Within anthropology, British social anthropology, which aligns itself with sociology—and frequently defines itself as "comparative sociology"—has in the last two decades drawn apart by emphasizing its aim of achieving a comprehensive social theory. In consequence a distinction is sometimes drawn between social and cultural anthropology. The latter is recognized as owing many of its emphases to Boas and would, according to the terms used in this paper, be characterized by the natural history approach. This is still the broad discipline first outlined by such men as Tylor and Waitz, and is referred to here as anthropology, without qualification. Social anthropology, in contrast, delimits its field more strictly and is characterized by the social philosophy approach.

In characterizing Boas' thought and field method as distinguished by the natural history approach, it is difficult to know to what extent my own posi-

tion, learned in part from Boas, influences the contrast being made. It would be fascinating to analyze his writings for a full statement of his own concept of man, but this task cannot be attempted here. Boas has meant many things to many people, and his influence is still hardly to be separated from his work. My awareness of the contrast has been sharpened by six years of close contact with certain British social anthropologists, and I owe a great debt to these British colleagues. Much that has been written concerning the two positions now seems to me to be peripheral to the essential differences in approach. The contrast as I present it may well suffer from over-generalization, but it seems to me fundamental to an understanding of Boas.

First, it is necessary to point out that one or the other approach is to be found in all the social sciences. They are often fused, or partially adapted, in the writings of a single social scientist, who may take first one position and then the other. It is only in the exceptional thinker that either position becomes clear and relatively self-contained, with its full implications realized. Because the approaches are generally implicit, they have little to do with the acceptance or rejection of explicit theories. When a position has been articulated it can be accepted for purposes of argument, and discussions concerning it can be exceedingly fruitful. When distinctions are implicit, on the other hand, communication becomes difficult and discourse on matters concerning them tends to evaporate in a haze of misunderstanding.

When such misunderstanding appears in anthropology, the difficulties are not due to differences in subject matter. It is not a question of knowing American Indian rather than, say, African data. Anthropologists are able to switch from one area to another and they have also been able to transfer their attention from primitive societies to modern industrial communities. Whether or not the anthropologist engages personally in research, he can foresee many of the problems which will arise, and discuss methods for dealing with them and the probable value of the results. Neither does the confusion arise from the importance which data as such are accorded. All anthropologists are empiricists in this sense, and all insist on the absolute importance of fieldwork; they also demand intensive studies carried out in anticipation of eventual comparison. Nor do differences in the recognition given to certain areas of investigation seem to me to constitute the main blocks to communication: the difficulties are not mainly due to such factors as the omission of linguistic data or a fondness for archeology.

Both in Great Britain and the Americas, anthropology continues its tradition of covering more than purely social phenomena. It includes archeology and physical anthropology and, outside of Britain, linguistics as well. In the United States, it is the only discipline to be included in the Social Science Research Council, the National Research Council (because of several of its aspects, including the biological), and the American Council of Learned Societies (because of its relations to the humanities). In Britain, it is always a moot point whether anthropology belongs with the disciplines of the Royal Society or those of the British Academy. Anthropology is becoming even

wider with the current interest in cross-disciplinary studies. Despite criticisms to the effect that the fields are becoming too specialized for one person to be able to control several of them, and that he who attempts more than one "is spreading himself too thin," the general scope seems still to be expanding.

Although many sciences have retained the evolutionary type of approach, that of anthropology was considerably modified by Boas. His anti-evolutionary emphasis need not be reviewed here in detail. It is obvious that his rejection did not apply to the facts of evolution, which he and his students continued to document in physical anthropology as well as in technological fields. Rather, it was directed against evolution as a doctrine. The acceptance of evolution had become so general that there was a predisposition to view field data in its terms. Boas' arguments were directed against this use of evolution as a basic premise capable of coloring observation and interpretation.

Distrust is easily voiced, but such a doctrine cannot be readily replaced. Social scientists have been hard-pressed to find suitable substitutes for traditional concepts. It is frequently said, for example, that the self and society may be considered aspects of a single whole. Social scientists are aware that the interactions they study may preclude or render meaningless the opposition between the individual and society, but analyses are generally made in terms of one or the other of these units, and cross-cultural comparisons tend to perpetuate the dichotomy. Boas himself seems to have accepted this particular tradition, although he demanded that the interaction be studied. His general position in regard to tradition was plain: the only corrective was to allow conclusions to follow from the data without the introduction of preconceived philosophical positions.

Distrust of authority takes a different form in the social philosophy approach. Although this approach follows the long and firmly established path of philosophy itself, it grew up in the atmosphere of anticlericalism. It was an assertion of independence from clerical, even from divine, control. Whether or not its exponents accepted the extreme position of *cognito ergo sum*, they looked for the essential quality of man in his own faculties rather than in those derived from outside himself. The social philosophy approach was attracted to evolutionary theory in part because this made the progress of society dependent upon only those forces which were internal to the social system itself. Unilineal social evolution has never been wholly abandoned by such thinkers. Man's confidence could be centered in man; and in today's terms, both rationalism and humanism belong in this tradition. Perhaps a more significant development is the recently heightened importance of logic. If reason is to be one of the principal analytical tools, it must be honed down to a fine edge; its own analysis becomes essential.

Exponents of the social philosophy approach follow Durkheim in concentrating on the relationships which exist between phenomena. This is where the challenge and the accomplishment lie. It is thus a matter of small moment that many of Durkheim's facts are inadequate or inaccurate. The system of relationships he has constructed transcends details. Such men insist that

phenomena be isolated, examined, and then related to each other. It is this threefold process to which their energies are applied, and the dexterity of their performance is in large part the measure which determines its acceptance. Validity thus derives less from verification than from the skillful manipulation of concepts.

The contrast between the two approaches is often described as though it boiled down to the significance of definition. A single issue of a recent publication carried two examples of contrasting attitudes toward the value of refinements in definition. One quotes the phrase of Trilling on the "useful ambiguity which attends the meaning of the word culture" (Lerner and Riesman 1955:67); the other speaks of the social anthropologist's endeavor "to employ a terminology which is completely unambiguous" (Leach 1955:82). The first centers attention on the descriptive material, and is content to leave the terminology fluid. The other emphasizes that science is discourse and must be precise in order to be valid. The first, or natural history, attitude is criticized for its lack of precision: "How can you talk about something when you cannot even say what it is?" And the social philosophy approach of the second is said by its critics to lead to a taxonomy which becomes more and more divorced from reality.

Boas' interest in classification was also reflected in attention to taxonomic systems. As he saw it, the problem was related to distrust of traditional concepts—in his case less a distrust of authority, for that he never accepted, than a deep suspicion of tradition as reflected in one's own thought processes.

> In natural sciences we are accustomed to demand a classification of phenomena expressed in a concise and unambiguous terminology. The same term should have the same meaning everywhere. We should like to see the same in anthropology. As long as we do not overstep the limits of one culture we are able to classify its features in a clear and definite terminology. We know what we mean by the terms family, state, government, etc. As soon as we overstep the limits of one culture we do not know in how far these may correspond to equivalent concepts. If we choose to apply our classification to alien cultures we may combine forms that do not belong together. The very rigidity of definition may lead to a misunderstanding of the essential problems involved. . . . If it is our serious purpose to understand the thoughts of a people the whole analysis of experience must be based on their concepts, not ours (Boas 1943:314).

To Boas, man was preeminently a thinking creature. Reliance on an outer world never led him to the position that culture exists of and by itself, but rather that thought be subjected to the same treatment as other phenomena. An exponent of the natural history approach once wrote me: "I argue all the time with people who are oriented in terms of social theory as to the need of having theory develop out of data and of constantly referring theory to data." And the key to the contrast in approaches is often described in different postulated relations between data and theory. But if one glances at the work in any area of anthropology—or of the other social sciences—one is impressed by the fact that all aim at precision in definition, and all relate theory to data. The distinction seems to lie instead in the amount of care taken to preclude preconceived notions.

Boas' effort to preclude traditional concepts had immediate effects on his ethnographic method. Perhaps his most typical contributions are the phonetic texts which he produced in quantity and which he urged others to produce. They were taken down verbatim, and the interlinear translations were made with the assistance of the informant. These come close to being exact samples of a people's thought. Boas was conscious of distortions which could occur through the introduction of a foreign or a sophisticated collector (Boas 1916: Introduction), but the texts supplied a nearly perfect record of a people's language and of the organization and style of their connected discourse. The value of the texts lies in part in the fact that they preserve literary patterns which are fast disappearing under white contact, but their greatest value, and the one on which Boas was most insistent, lay in their character as raw data. Once they were published, conclusions drawn from them could be checked, and they also furnished material for other investigators to use in analyses not envisaged by their collector. The texts were objective in the sense that they were always available for reanalysis. Boas stressed that information should be collected and presented in as great detail and as free of outside bias as possible. The first duty was to give field data in the people's own terms: ". . . the whole analysis of experience must be based on their concepts, not ours."

Both approaches draw on the philosophy of science for validation and clarification. Thus exponents of the natural history approach are apt to think of field situations as counterparts of the laboratories of other scientists: the social scientist in contact with his data is paralleling the work of the laboratory technician. They know he works under conditions which make direct experimentation impossible, but he attempts to make his observations accurate and in his comparisons he finds factors which may be effectively employed as controls. The social philosopher, on the other hand, thinks of scientific activity primarily in terms of the testing of hypotheses. He conceives his first task to be the careful consideration of alternative hypotheses: data are then brought to bear on them so that they may be accepted or rejected, modified or refined. The field situation or his collection of facts is thus a testing ground, and its value is proportional to the pertinence of the hypothesis he checks with it. A worker in the one approach may be happy in amassing data, with a minimum of generalization; a worker in the other may be equally content to elaborate his hypotheses. One believes his methods are leading toward an accurate, or scientific, picture of actuality; the other may say, "scientifically, that is, hypothetically." As a matter of fact, the total demands of both positions include data and generalization, hypothesis and observation, but the extremes cited here occur with amazing regularity.

When one asks that theory be referred to data, or that conclusions follow as they will from data, this is often construed as a process similar to that involved in testing an hypothesis. Actually this is not a very accurate description of what is intended. Boas taught his students statistics and phonetics as tools for handling biological series and language, but the greatest lesson we learned was that data had an order of their own. This came out most clearly in the classes he conducted in the Eskimo and Kwakiutl languages. Part of the

time he acted as informant and let the class work out a grammatical structure by questioning. The grammar was there and it rested with us to ask for translations which would reveal it. By giving only the raw data of speech, without formulation, Boas made us aware that our task was to discover the order we knew to be there. I well remember Boas' delight when I finally worked out the pattern of an Inca textile which at first sight had seemed to be only a jumble of elements with no pattern at all. This was not in itself important, but it could only be done by working with the materials in their own terms. The series of hypotheses which had to be tested could arise only as one worked with the data; none of them could have been formulated beforehand. It should also be recognized that once the data were arranged in the proper order, like the forms of a pronoun or the positions of a locative, the structure became self-evident.

A curious result of this view is to smudge over many common distinctions. For example, the opposition between abstract and concrete almost completely disappears. Whatever the research worker may claim to be his own beliefs concerning the nature of reality, he faces his data, whether abstract or concrete, as though they were equally susceptible to the same treatment. If firearms and sanctions against homicide are introduced simultaneously, the firearms may not be omitted from consideration because they are of a different order of being from sanctions. Both items are innovations due to contact, both may influence economic pursuits or social structure. It may be difficult to assess their relative importance in the new situation which they combine to create. Coming on the situation late in time, therefore, the research worker may have to abandon any preference as to the choice of material, and embark on a study of the relative efficiency of bows and arrows and guns, which was entirely beyond the original plan of his project.

A frequent cause of lack of communication is this apparently indiscriminate acceptance of fact. The social philosophy approach is deeply concerned with the nature of the material it is called upon to examine. It may make preliminary judgments as to the relative validity of sources of information. The natural history approach, on the contrary, begins by treating all fact as commensurable. It mistrusts judgments as to validity and is deeply concerned with the precision of interpretation. Thus, the one approach refers the expressed opinion of an informant to standards of veracity: Why did he say this, rather than that? Is what he said true? The other tends to take the opinion as given, compares it with another opinion expressed under similar circumstances, and asks questions about the significance of their similarity or dissimilarity. Exponents of the two approaches must be conscious of this difference. Otherwise, the social philosophy approach seems to have a naive obsession with "truth," and the natural history approach is thought of as naively gullible. Whereas the first accepts its premises as secure and questions its material, the other finds its security in the inviolability of its data and believes that if data are completely described they can be treated on one plane as hard fact.

One of the great bugaboos of the natural history approach is ethnocentrism. In contrast, social philosophy is little concerned with this factor. Suspicion of ethnocentricism is a natural concomitant of Boas' position, as outlined above, and its presence in modern thought is due in large part to him. This is perhaps not the least of his contributions, but it needs further elaboration here in relation to his ethnographic method. If I understand his position correctly, interpretation should enter the picture only after data have been collected. Raw data must be kept as free of outside interpretation as possible. It therefore followed that information gathered for a particular purpose became suspect, for selection in itself suggested distortion.

This does not mean that Boas never had problems in mind when he collected material. For instance, he wrote in 1920:

> One of the greatest obstacles to a clear understanding of the social organization of the Kwakiutl is the general confusion caused by the reduction in numbers of the tribe. I have tried to clear up the situation by recording the histories of a number of families in all possible detail (Boas 1920:111).

The interesting point here is the choice of the data selected for concentrated effort. The "histories of a number of families" constitute in themselves a block of information which can be used for as many purposes as can a collection of phonetic texts. They are raw data in the same sense. It would be useful to have such data included in any field study, but the selection here was determined by the local situation and was tailored to fit the demands of the particular inquiry. It did not grow out of hypotheses conceived in the culture of the investigator, and it did not overstep cultural boundaries. Selection of data, therefore, is suspect only if it does either of these things.

Systematic work was essential for obtaining the material needed to illuminate any problem. Boas never believed in the haphazard collection of field data. So far as I know, he left no explicit statement of his field techniques, but the nature of much of his field material suggests that he allowed informants a great deal of freedom in discussing topics of their own choice. This would be thoroughly consistent with his position as described here. Many of his data probably could not have been collected in response to direct questions. His effort to free thought from its traditional aspects would alert him to the fact that, coming from another culture, he might be unable to phrase the proper questions. At the start of work with an unfamiliar culture the research worker simply does not know enough for that, but he can begin with the most general inquiries and remain wholly systematic.

Boas' insistence on the integration of culture has direct bearing on this point, and it may not be irrelevant to recall an incident which occurred at one of my first attendances at the famous Wednesday afternoon seminars at Columbia. I do not remember what the paper had been, but discussion centered on the impossibility of foreseeing the ways in which one part of culture would be integrated with any other. In order to study art, therefore, one would

have also to sample economics, the position of the artist in the prestige system, ritual, and so on. Furthermore, the integration of culture was such that the study of art would not be complete until all the other aspects of culture had been thoroughly investigated. Although I agreed with this in principle, I ventured to suggest that if the main problem was art, the investigator would concentrate on artistic matters such as techniques, style, and design elements. None of the students would allow this, and I was properly squelched. Afterward, going down in the elevator, Boas happened to stand near me, and he leaned over and said, "There *is* a thing called art." Nothing more was needed. The young student had been encouraged, and perspective had been reestablished.

The position seems actually to be that although both the investigator's previous unfamiliarity with the culture and his anticipation of its integration lead toward free interviews, concentration on particular problems is not only permissible but may be expected, particularly in the later stages of fieldwork. Once the problems have been seen in terms of the new culture, fields for study may be isolated. Systematic work can then be devoted to accumulation of the relevant raw data. Experience has also shown that certain topics such as kinship systems must always be investigated, and a fieldworker in a new area would therefore expect to do systematic work along these lines. In addition, Boas' emphasis on systematic fieldwork led to the collection of whatever data became available. If one found an informant to be particularly well versed in a subject, one concentrated on that subject, getting everything possible from him and following through with other informants, observations, and the like, even though the immediate usefulness of the material might not be apparent. Precision could come only from data systematically gathered ". . . in all possible detail."

This exhaustive collection of data which seems at the time to have little or no connection with any specific problem is peculiarly a feature of the natural history approach. Interest lies not mainly in systems per se, but in "the surrounding world." There is a fascination in following the details of a subject just for its intrinsic interest, and there is also the knowledge that, once accumulated, such systematic data will have value—sometimes in wholly unexpected directions. Masses of data may therefore be worked over with no clear knowledge of what is to be gained at the end. A new hypothesis or a new slant on an old problem will "emerge" or be "revealed" or "suggested." The data will "speak for themselves." This is the procedure by which the exponent of the natural history approach prefers to arrive at hypotheses: they do not come from systematic thought but from systematically ordered data.

The integration of culture may be thought of as a typical hypothesis of the natural history approach. The "conclusion" that cultures form more or less integrated wholes derives not from thoughtful analyses of our own cultural materials but from data collected over many years on a number of cultures. Comparison then "establishes" similarities which occur widely, and the hypothesis is ready for reexamination and refinement. It is not hypothetical in

any usual sense of the word. It is rather a scientific generalization on regularities, similar to any which might be formulated in, say, astronomy. Boas always emphasized that the data of anthropology were different in many respects from those of the natural sciences, notably because of the presence of historical factors. He was equally clear, however, in stating that a people's actions would be the same whether or not they knew anything of the historical background. Dialects are spoken without the speakers being aware of the phonetic shifts which have occurred over time. Although culture change is therefore of great importance for ethnological analysis, I do not see that consideration of such change directly affected Boas' ethnographic method.

The same objections which can be made to selection of field data according to a preconceived scheme can also be brought to bear on a number of favorite sociological techniques. For example, the questionnaire is designed to sample opinion and to elicit responses on topics deeply imbedded in cultural values. It presupposes knowledge of the culture, and is pertinent to problems arising in the solution of imbalances. To transfer such instruments of investigation unchanged to other cultures is obviously unsound. But to construct similarly useful sets of questions for an alien culture, it would be necessary to have the same careful background analyses which the sociologist has been building up over the years; to attempt to devise a truly cross-cultural questionnaire would add tremendously to the difficulties. Questionnaires thus grow out of the investigator's own culture, and they overstep cultural boundaries. The second of these objections really constitutes the main point because, although Boas could have devised a questionnaire for Kwakiutl potlatch procedures, he would clearly have regarded it as inappropriate for administration to men who hold seats on the New York Stock Exchange. Investigation of the common features in these two cultural situations would demand a third, specially devised questionnaire—or other techniques—and it could follow only on parallel, prior investigation. Actually, Boas followed earlier anthropologists in using lists of questions. In his case the lists were small, but a fieldworker going to any part of the Northwest Coast would be asked, for instance, to supply the local word for "seal." These questions, far from overstepping cultural boundaries, were designed to help discover exactly where they lay.

The social anthropologist is apt to stress the importance of observation in his fieldwork; he tends to suspect informants' statements and to rely largely on those data which derive from his personal observation. This emphasis is foreign to Boas' approach. First, one of his prime interests was in meaning: he wanted to know not only what was done but why the persons thought they were doing it. Second, he was acutely aware of the extent to which perception is influenced by personal factors; people see what they expect to see, and interpret what they have seen in the light of their own previous experiences. All the arguments against ethnocentricism here come into play. Even the trained observer has constantly to be on guard against possible inaccuracies and omissions in his own records. Boas was always too self-critical to rely completely on his own observations. He needed the documentation of the text, the family

history, to test his own precision. His half century of work with the Kwakiutl was increasingly intensive, but it was marked by constant reference back to early materials, checking and rechecking. Of course he depended on observation, but he took that for granted; it was typical of him that he expressed the limitations of a method more clearly than its values, which he took to be self evident.

The same demand for raw data determined Boas' relationship to such fieldworkers as Henry W. Tate, who collected the texts for *Tsimshian Mythology*, George Hunt and James A. Teit. It required the trained anthropologist to work with and analyze field data, but valuable material could be gathered by someone trained in a few relatively simple techniques. These men were technicians in the sense that the laboratory sciences have technicians. When so much has to be done, some of the tasks can be delegated. Hunt was a Kwakiutl; Teit had close ties with the people from whom he began to collect information, but later he also worked with relative strangers. Part of their value to anthropology lay in the fact that each man spoke the native language and had connections which enabled him to make personal contacts readily. But Teit's importance, especially in his later work, lay principally in the fact that here was a man on the spot, able and willing to continue the anthropologist's work. Folklore was naturally adapted to this, since a great deal of its collection could be described as relatively routine, but Teit's work was not limited to folklore.

It is interesting that this aspect of Boas' ethnographic method has not been more widely adopted. I suspect that this is partially because of the labor involved in editing material gathered by technicians, in going over it with the care necessary to make it part of one's own data. Most anthropologists find it difficult enough to go through their own notes. But I suspect also that as anthropology became an increasingly important university subject, students were employed more and more to augment earlier work. This served the dual purpose of adding information and giving students field experience. Some students, such as Ella Deloria, were as well fitted for fieldwork among their people as Hunt had been, but her work with the Sioux is not comparable to his among the Kwakiutl because she brings to her research the full training of the anthropologist. Remarkably little attention has been given to the development of field technicians who are not themselves training to become anthropologists.

From the time of *The Central Eskimo*, Boas always included with his own data any facts available from other sources. This did not imply that his use of such sources was uncritical. He felt that the more information one had, the more correct could be one's conclusions, and he often corrected a statement or an opinion earlier expressed in the literature.

> From observations made by Captain Spicer, of Groton, Conn., and information obtained from the Eskimo, we learn that the whole of the eastern part of Fox basin is extremely shallow . . . (Boas 1888:416).

But one judges each statement on its own merits, and this acceptance of supported Eskimo testimony is followed by:

> Though the Eskimo assert that the discovery of Lake Nettelling is of recent date . . . this assertion is not trustworthy, for with them almost every historical tradition is supposed to have originated a comparatively short time ago. I was told, for instance, . . . (Boas 1888:430).

Boas regarded data from others as critically as he did his own observations, and his criticism in each case was based on the same reasons.

Had Boas reached the eastern part of Fox basin, he would have been able to verify its depth; lacking first-hand observation, he came to a decision on the information available. It is strange that Boas is sometimes said to have failed in the formulation of hypotheses. Every page of his descriptive work is full of such conclusions drawn from data. Each was viewed as subject to correction, and each was therefore regarded as tentative until fresh evidence came to hand. As his work became more intensive, fewer facts gathered by others could be brought in as relevant. But his work was extensive as well, and his conclusions in theoretical matters were based on information drawn from a variety of sources. It is true, however, that these conclusions led only to accuracy and precision in description and to depth of understanding. Interest in an abstract and universal truth which lay outside of the material world was foreign to Boas' thought, and hypotheses which aimed at establishment of such truth were absent from his ethnographic approach.

Although Boas was critical of information supplied by local residents, his attitude toward informants was not generally suspicious. In no other phase of their research are the extreme positions of the natural history and social philosophy approaches more distinct than in their relations to informants. During and just after the last war, several efforts were made to substitute some other word for "informant" because of the implied relation between it and "informing," with its unpleasant connotations. The close tie between an anthropologist and his informants is of such a special kind, however, that other words with other overtones of meaning already established failed to express what was intended, and the term has continued in use. Although the anthropologist is learning from his informant, the relation is clearly not that of pupil and teacher. Nor is the anthropologist asking for secret information; he considers important everyday matters which the informant would not ordinarily think about. Once the relationship is established, both anthropologist and informant work together on the material—one contributing his skills, the other his knowledge and personal experience.

A study of ethnographic method could dwell at length on the various methods of conducting interviews. The kinds of personal interactions involved appear to influence directly the nature of the information obtained. The various interviewing techniques may all find a place at one time or another in the anthropologist's repertoire, and he may be more or less aware of their effects. They are of particular importance with supplementary informants who are used for comparatively short periods. Boas' own fieldwork, however, is distinguished by concentrated work with a few informants. This might continue over months or even years, and interviewing techniques lose much of their significance in the light of such protracted contact. Work with informants

means work of this intensive kind and it is not generally comparable to ordinary interviewing.

Boas had early recognized that some types of information do not lend themselves to mathematical treatment (Boas 1938:22). This was particularly true of ideas and concepts, and to the extent that Boas' work was devoted to establishing a people's own concepts he was not interested in applying the statistical criteria he used so effectively in his studies of growth. To get the depth of understanding he required meant submerging his thinking in that of another. It meant learning to think in another's terms and to view the world through another's eyes. The most intimate knowledge of an informant's thought processes was mandatory and could only be obtained by intensive work over a long period. Important concepts and strange viewpoints had to be checked with other material and with a number of informants; supplementary information had to be obtained elsewhere. But Boas conceived of his main task as the adoption of an informant's mode of thought while retaining full use of his own critical faculties. For this, sampling and other semi-mathematical techniques were of no immediate assistance, and they played little part in his ethnographic method.

The custom of concentrating on a few informants may have arisen in work with American Indians in part because of the urgent need felt by many early anthropologists to obtain and preserve information before it was too late. Conditions seemed to be changing so rapidly that in a few years the old ways would be gone forever. There was also the practical consideration that most people are too busy to spend much time with an anthropologist, however willing they may be, and one is obliged to work with the few who already have time on their hands or to create circumstances which make time available. But neither the imminence of change nor practical considerations lay behind Boas' choice of method; it was determined by his whole approach. He could obtain the kind of knowledge he wanted only by concentrating his work with a few persons.

Boas was little concerned whether an informant was "typical" or not; this was a pseudo-statistical concept which he disposed of most effectively in his discussions of race. Surely any active participant in a society must be thinking and generally behaving in ways appropriate to that society. Boas' interest lay in uncovering the premises on which such a person operated, and if it were not possible to work with a representative of every specialized role in a society, at least every person had ideas as to what those roles were. Intensive work with an informant means reviewing with him every aspect of his culture, retaining his emphases, noting his omissions, and maintaining the relationships he establishes. Work should be done with such precision that one could construct a house from the informant's description of a house. Although one can generally see a house, observation must be combined with data from informants, for what an informant sees in a house—designs and ritual association, symbols and values—will be quite different from what the untutored anthropologist will see in it. This ideal of complete work with an informant may seldom be realized

in practice, yet the method is clear-cut and applicable to any society of any complexity. Of course data obtained in this way must be augmented by many other kinds of information, but there is no substitute for exhaustive and intensive work with informants.

A misstatement of fact can be serious in an interview. The person being interviewed may intentionally distort the true picture for any one of a number of reasons and the investigator must allow for such factors in assessing his results. Falsehood of this sort is impossible to maintain in the protracted anthropologist-informant relationship. The complexity of fully recorded data goes far beyond any tests of veracity. Thus, if an informant says that something happens, whereas the anthropologist observes the opposite, this cannot simply be disposed of as a lie; it poses a problem which must be given further attention. In the same way, consistent distortion may be a clue to important facets of behavior, to be noted and given their appropriate weight. Boas' field material gives no evidence of moral judgments of his informants or of the customs of their societies.

Many anthropologists who studied under Boas have relied heavily on "main" informants. Their thinking has been conditioned by intensive work with one or two persons with whom they could establish rapport, and with whom it was possible to work for long periods. Although it seems a natural outgrowth of Boas' work, this type of relationship does not seem characteristic of it. Boas himself seems always to have retained considerable objectivity. He worked with informants in order to obtain a particular kind of information, and the data themselves remained foremost in his interest. This lends his work a curious detachment which is slightly repellent to social scientists who are absorbed in people. But it must be remembered that it was the whole world with which Boas was fascinated. People were part of it, but they were not the reason for its being. They represented no culmination of creative effort, no end point of development.

Another aspect of this same broad view of all phenomena underlay Boas' attitude to individuals.

> The habit of identifying an individual with a class, owing to his bodily appearance, language, or manners, has always seemed to me a survival of barbaric, or rather of primitive, habits of mind. . . . There are too few among us who are willing to forget completely that a particular person is a Negro, a Jew, . . . and to judge him as an individual (Boas 1938:24).

Acceptance of class standards and national stereotypes naturally seemed to him a form of subservience to traditional concepts. I believe that his whole position on race was determined not only by the factual aspects of scientific inquiry but also by his conviction that obedience to the dictates of racial prejudice constituted a violation of freedom of thought. One must determine for oneself the attributes of individuals.

Boas carried this attitude with him in the field. Not only did he refrain from preconceived judgments but he also met each man without condescension, with

no sense of the absolute superiority of the Western culture which he himself represented. His standards were high, but they were for individuals, and it is particularly to be noted that he found individuals in the field who met those standards, whom he could meet on his own intellectual terms. The experience of other anthropologists confirms this, and one of the best documented facts in cross-cultural work is the presence in many cultures of persons capable of clear judgments and dispassionate opinions on matters close to their own and their society's interests. Cultural differences may remain, but such persons must be judged on a par with the investigator. Although Boas felt no hesitation in attributing positive value to certain actions and modes of thought, his work was not normative; it was given its unique force by his refusal to abide by cultural prejudices and by his recognition of the universal occurrence of human qualities. His capacity for maintaining both these positions relates directly to the natural history approach which he so well represented, and which seems as pertinent as ever to the problems of the social sciences.

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# *The Understanding of the Kwakiutl*

HELEN CODERE

BOAS' Kwakiutl and Northwest Coast publications total more than 10,000 printed pages, written over a period of almost six decades, and the attempt to generalize about how Boas saw Kwakiutl culture, non-Kwakiutl Northwest Coast cultures, and their interrelationships is reasonable only because of the consistency of his informing ideas and resultant actions.

The outstanding feature of Boas' Kwakiutl and Northwest Coast work is the emphasis upon the symbolic aspects of the cultures. Most of Boas' major works on individual Northwest Coast cultures and on comparative Northwest Coast cultural contexts deal with mythology, language, and art, in that order. While all human culture can be thought of as symbolic behavior, the distinction between material and nonmaterial culture does have analytical meaning. If some aspects of culture, such as social and economic organization, must be placed between the material and nonmaterial poles, technology can nevertheless be clearly assigned to the former and myth, language, and art can best be seen as symbolic—even as symbolic systems for which no transformation system has yet been devised to clarify their relation to the other aspects and structures of the culture.

The symbolic emphasis in Boas' Kwakiutl work and thought did not cause him to slight other types of description and analysis, any more than Sapir's central linguistic interests delimited his work. Of course, this specialization in all aspects of Kwakiutl culture could not have been duplicated for Tlingit, Tsimshian, Haida, Bella Coola, Bella Bella, Nootka, Coast Salish, and the neighboring peoples of the South and the Interior. The remarkable thing is the number and the scope of "symbolic" studies carried out for these other Northwest Coast cultures, and the size and complexity of the comparative analyses.

Though it is a risky procedure, I have endeavored to sort out "Major Works" from Boas' Northwest Coast bibliography and have tabulated them with specifications on their length and general character (see Table 1). The criteria were: (1) length, over 100 pages in all but two cases; (2) general or monographic treatment, and of not too limited intent, breadth and depth; (3) that the work was not superseded by later work. A fourth and subjective criterion was the degree to which control of the data and ideas of these volumes seems to be required in order to do Kwakiutl and Northwest Coast work. While this fourth criterion neither included nor excluded the works listed on Table 1, it perhaps adds to the connotations of "Major" in the heading of the table.

The remarkable feature revealed by the table is not only the proportionately large amount of symbolic cultural material, but also the relatively small amount of nonsymbolic material for any culture but Kwakiutl. Only about a third of the *Chinook Texts* (1894), a very small fraction of *Tsimshian Mythology*

TABLE 1

BOAS' MAJOR NORTHWEST COAST PUBLICATIONS: THEIR SIZE AND CHARACTER

| DATE | TITLE | PAGES | CHARACTER* | | |
|---|---|---|---|---|---|
| | | | Kwakiutl | Non-symbolic | Symbolic† |
| 1894 | Chinook Texts | 278 | | + | M |
| 1895 | Indianische Sagen | 363 | K112pp | | M |
| 1897 | The Social Organization and the Secret Societies of the Kwakiutl Indians | 427 | K | + | MLA |
| 1898 | The Mythology of the Bella Coola Indians | 104 | | | M |
| 1901 | Kathlamet Texts | 261 | | | M |
| 1902 | Tsimshian Texts | 244 | | | M |
| 1905 | Kwakiutl Texts | 532 | K | + | M |
| 1906 | Kwakiutl Texts—Second Series | 269 | K | | M |
| 1909 | The Kwakiutl of Vancouver Island | 221 | K | + | |
| 1910 | Kwakiutl Tales | 495 | K | | M |
| 1910 | Tsimshian, An Illustrative Sketch | 139 | | | L |
| 1910 | Kwakiutl, An Illustrative Sketch | 135 | K | | L |
| 1910 | Chinook | 119 | | | L |
| 1912 | Tsimshian Texts, New Series | 219 | | | M |
| 1916 | Tsimshian Mythology | 1037 | (K)‡ | (+) | M |
| 1917 | Folk Tales of Salishan and Sahaptin Tribes | 205 | | | M |
| 1918 | Kutenai Tales | 387 | | | M |
| 1921 | Ethnology of the Kwakiutl | 1481 | K | + | (A37ppL75pp) |
| 1925 | Contributions to the Ethnology of the Kwakiutl | 310 | K | + | |
| 1926 | Grammar of the Kutenai Language; and Additional Notes on the Kutenai Language | 84 | | | L |
| 1927 | Primitive Art. Chapter VI. | 105 | | | A |
| 1928 | Bella Bella Texts | 291 | | (+) 37pp | M172ppL132pp |
| 1930 | Religion of the Kwakiutl | 284 | K | + | |
| 1932 | Bella Bella Tales | 178 | | | M |
| 1932 | Current Beliefs of the Kwakiutl Indians | 83 | K | + | |
| 1934 | Geographical Names of the Kwakiutl Indians | 83 | K | + | ? |
| 1935 | Kwakiutl Tales, New Series | 230 | K | | M |
| 1935 | Kwakiutl Culture as Reflected in Mythology | 190 | K | | M |
| 1943 | Kwakiutl Tales | 224 | K | | M |
| 1947 | Kwakiutl Grammar (Posthumous) | 177 | K | | L |
| 1894–1947 | TOTAL PAGES§ | 8,978 | 5,255 | | |

* Parentheses signify a minor and subordinate amount of material compared to the notation of material-type that stands without parentheses.

† Symbolic material is defined here as that having to do with mythology, language or art and their initial letters are used on the chart. This definition of symbolic material is extreme but it is made with the desire to show the character of these works without exaggeration.

‡ In *Tsimshian Mythology* about the only nonsymbolic Kwakiutl material consists of two pages of Kwakiutl kinship terms (494–5).

§ It was not possible to give the total number of pages of symbolic and nonsymbolic material, since some of the sources were not available for detailed checking.

(1916) and about a twelfth of *Bella Bella Texts* (1928) form the exception to otherwise almost complete preoccupation with symbolic material. The remainder of this paper will trace the development of Boas' preoccupation with

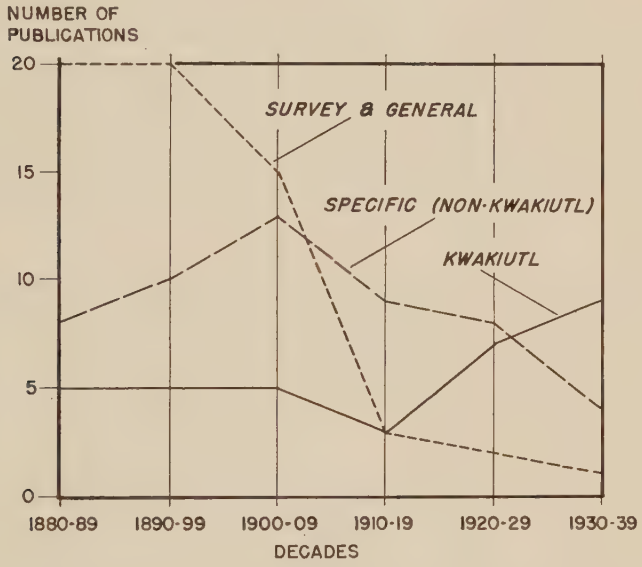
symbolic materials during the course of his Kwakiutl and Northwest Coast work, and will discuss the resultant understanding of Kwakiutl culture and other Northwest Coast cultures.

## THE COURSE OF BOAS' NORTHWEST COAST WORK 1885–1942

Before examining Boas' informing ideas and theories, and the nature of his Northwest Coast and Kwakiutl work in relation to them, it seems appropriate to consider some objective data on the course of his work and to separate this from any detailed consideration of its character.

First, there are definite trends and interrelations between survey and general Northwest Coast work, work specific to some non-Kwakiutl Northwest

Changing Character of Boas' Northwest Coast Publications




*Note.* Totals for decades. Totals are approximate except for the Kwakiutl publications because duplications have been omitted (e.g. the numerous brief papers explaining the plan and the course of the work of the Jesup Expedition, works published separately in German and English and works published separately as well as in such volumes as the *Handbook of American Indian Languages* [1911]).

Coast culture, and work on Kwakiutl. While the assignment of a work to one of these categories is often difficult and totals are approximate because of the existence of many duplications or near duplications, the results are fairly clear (Chart 1). From 1885 to 1899, survey and general or generalized Northwest Coast works predominate; specific and detailed studies of cultures other than Kwakiutl exceed Kwakiutl studies and increase sharply during the period. During the next two decades, from 1910–1929, specific non-Kwakiutl studies remain numerous and there is a sharp decrease in survey and general

work. From 1920 on, Kwakiutl works, mostly major works, increase and predominate, specific non-Kwakiutl works decline, and survey and general works are few. This general course was explained by Boas in his own review of his work (The Kwakiutl of Vancouver Island 1909:307–309). He said that in spite of his "irresistible attraction" to the Northwest Coast and some degree of preparation for his first visit in 1886, "The first impression, however, was that of bewildering confusion, and before it was possible to attack the more interesting problems, the relationship of tribes had to be cleared up" (1909: 307). He seems to have proceeded exactly as planned in working up general background materials, and to have settled on his Kwakiutl specialization by 1892, for no other specific group seems to have received as much of his attention by that date.

Second, his Kwakiutl specialization should be understood as primarily a specialization in Southern Kwakiutl culture. His work began with the Bella Bella (Northern Kwakiutl) in 1886 but he did not publish specifically on them again until 1928 and 1932, although Bella Bella mythological and linguistic data are introduced comparatively in such publications as *The Social Organization and Secret Societies of the Kwakiutl Indians* (1887:427–430; 660–4) and *Tsimshian Mythology* (1916:883–888). In these publications the Bella Bella seem to be of no greater importance than the Nootka, Haida, and Bella Coola, none of which seem to receive as much special or comparative consideration as Tsimshian.

Third, although Boas produced major synthesizing and comparative studies of all the Northwest Coast cultures in *Indianische Sagen* (1895), *Tsimshian Mythology* (1916), and *Primitive Art* (1927), as well as smaller studies such as "The Social Organization of the Tribes of the Northwest Coast" (1924); and although many of his Kwakiutl publications, especially the later ones, contained much cross-cultural material and analysis such as that to be found in *Kwakiutl Culture as Reflected in Mythology* (1935), he never finished his Kwakiutl work. As early as 1900, according to a statement he made some years later, he was planning "a general ethnological discussion of the [Kwakiutl] material contained in these volumes" (1921:45). It is not certain that his unpublished *Kwakiutl Ethnography* would be the work he planned, as its contents do not form a "general ethnological discussion" in the fullest sense of the phrase.

## AFFECT, IDEA, AND THEORY IN BOAS' NORTHWEST COAST WORK

In Boas' Northwest Coast work the affective, ideological, and theoretical are consistent, as are the procedures that develop from them.

His Northwest Coast interest began with his affective response to the art of the people:

> . . . my fancy was first struck by the flight of imagination exhibited in the works of art of the British Columbians as compared to the severe sobriety of the Eastern Eskimo. From the fragmentary notes furnished by Captain Jacobsen we divine what

a wealth of thought lay hidden behind the grotesque masks and the elaborately decorated utensils of these tribes (1909:307).

He soon formulated and applied certain guiding ideas for his Northwest Coast work, and it is of interest that his desire to find out about the "wealth of thought [that] lay behind" cultural facts underlies his procedures which were thorough, meticulous, and comprehensive. As Herskovits states, culture for Boas was the manifestation of the "mental life" of man (1953:72) and it would follow that Boas should—and did—commit himself to working for and with the most detailed, complete, and accurately recorded cultural material. It will be argued later that Boas' emphasis on Northwest Coast symbolic material is intimately connected with both his point of view and his procedures; here it is useful to show that Boas applied these ideas and procedures to his Northwest Coast and Kwakiutl work:

> During these years, from 1886 to 1892, the information that was accumulating seemed to show that under the present conditions the Kwakiutl and Nootka offered the most promising fields of research, partly because they were less affected by the whites than other tribes, partly because they exhibited peculiar transitional stages, in which newly acquired customs appeared to have assumed novel significance—a condition favorable to the study of the psychological and historical processes which are characteristic of the cultural development of comparatively primitive tribes (1909:308).
>
> . . . It fell naturally to my share [of the Jesup Expedition work] to continue the ethnological investigation of the tribes of Vancouver Island. It seemed to me well to make the leading point of view of my discussion, on the one hand an investigation of the historical relations of the tribes to their neighbors, on the other hand a presentation of the culture as it appears to the Indian himself. For this reason I have spared no trouble to collect descriptions of customs and beliefs in the language of the Indian, because in these the points that seem important to him are emphasized, and the almost unavoidable distortion contained in the descriptions given by the casual visitor and student is eliminated (1909:309).

Work after work is in this spirit, and meets its exacting standards. In one of the most meticulous of works, *Geographical Names of the Kwakiutl Indians*, Boas gives as the reason for its existence "that geographical names being an expression of the mental character of each people and each period, reflect their cultural life and the line of development belonging to each cultural area" (1934:9). In this case time seems to have proved his point, for shortly after the book was published a Kwakiutl fisherman specializing in commercial salmon fishing, as most were by then, took his sons halibut fishing with Boas' book to guide him to the halibut grounds; and by 1951 no Kwakiutl could have reproduced any of the maps, even those of his own immediate locality, in full detail.

Boas' characteristic theoretical viewpoint developed along with his Northwest Coast work, particularly as it embodied his fascination with the attempt to discover the mental life behind cultural phenomena. In a 1930 discussion of *Geistewissenschaften*, he is discouraging about the possibility of discovering general laws:

> . . . Generalizations will be the more significant the closer we adhere to definite forms. The attempts to reduce all social phenomena to a closed system of laws applicable to every society and explaining its structure and history do not seem a promising undertaking (1930:268).

In 1936 he makes fully articulate his feeling for detailed cultural material and the theoretical point of view that necessarily resulted.

> . . . I aligned myself clearly with those who are motivated by the affective appeal of a phenomenon that impresses us as a unit, although its elements may be irreducible to a common cause (1940:305).

And in the same source: "Absolute systems of phenomena as complex as those of culture are impossible. They will always be reflections of our own culture" (1940:311). This theoretical position seems to be part of an interrelated whole which consisted of his feelings about cultural phenomena, his ideas about the knowledge needed to make discovery possible, and the Kwakiutl and Northwest Coast works themselves, for each work seems to have a place in a great but uncompleted plan to understand man's mental life by studying the culture it created and in which it was reflected. These generalizations can have full force only if Boas' accomplishments are considered along with his brief and infrequent statements about himself and his work. His works form a reflection of his feelings, his "mental life," and such comprehensive and meticulous work must reflect consistent affection for human beings and fascination with their creations. Otherwise patience alone would have produced little, and energy would have had no direction.

## BOAS' VIEW OF KWAKIUTL CULTURE

It is not possible to present a synthesized account of Kwakiutl culture based upon Boas' works. This should await publication of his *Kwakiutl Ethnography* and should contain full documentary paraphernalia, since complex matters of interpretation are involved, as was seen in the recent controversy on Kwakiutl social organization (Ray 1955, 1956; Kroeber 1956; Lowie 1956; Herskovits 1956; Codere 1957). However, a number of general points about Boas' view of Kwakiutl culture can be made at this time and in this compass.

Boas concentrated his interest on the Kwakiutl culture that existed between 1850 and 1920, and his interest in its dynamics centered on the pre-1850 period—on the discovery of the cultural-historical processes that formed Kwakiutl culture of what I have termed the "Potlatch Period." In his studies of the Potlatch Period, he tirelessly traced historical clues and reconstructed earlier Kwakiutl culture on the basis of most probable relationships and developments. There seems to be no important aspect of the culture that he did not work at in this fashion: art, ceremonialism, social and economic organization, village settlement pattern and political organization, the potlatch, literature, religion, language, and so on.

As an illustration of these typical concerns in Boas' Kwakiutl work, I

should like to present in some detail the pre-1850 picture of supernaturalism and ceremonialism according to reconstructions made by Boas at various times. Responsibility for synthesis and interpretation will have to be mine in many cases.

At first, or as far back as the reconstruction can be pushed, the Kwakiutl lived in patrilineal descent groups (Boas 1897:336; 1924:329–330), shared the widespread North American Indian guardian-spirit concept and type of supernatural belief (1897:336–337; 662), general North American and Northwest Coast literary materials and motifs (1897:662–663), and were probably much concerned with warfare. Boas states that the Kwakiutl originated the ceremonials that were later to spread over the whole area (1897:660–661) and that the rituals and the secret societies which performed them "had their origin in methods of warfare" (1897:664). The Kwakiutl then got the idea of hereditary crests from the matrilineal people of the north (Boas 1897:336–337; 662; 1920:336–368; 1924:329–330) and a series of complexly interrelated developments ensued. Guardian spirits became crests inherited through the female line by the device of a man bestowing on his son-in-law the right to use a ceremony; the son-in-law would have one of his relatives reenact it and ultimately turn it over to his own son-in-law. Although the relative who reenacted the guardian spirit incident had to be initiated into the secret society appropriate for that type of ceremonial (the initiation involved the seclusion of the initiate while presumably under the influence of the spirit and his being "tamed" or freed from the spirit's hold), the religious significance of the experience and ceremony seems to have been attenuated and no longer to have resembled the individual, and often intense, nature of guardian spirit relationships so common in the rest of North America. Family histories and guardian spirit legends, insofar as they were separate from family histories, were reworked so as to validate the supernatural ancestor and his crests for each numaym and to record the passing of the now altered guardian spirit ceremonials (as crests) from a man to his son-in-law (Boas 1897:337, 662–663). Yet it was still possible to obtain a man's family names, rank, and such ceremonial crests as he had by killing him in warfare, but it is not clear how a man would know the details of the winter dance ceremonial he obtained in this manner. Usually the father-in-law had the ceremony performed for the son-in-law's benefit. Since a man did put on the ceremonials he obtained in warfare, it seems justifiable to conclude that their connection with warfare was still very intimate. Obtaining them in war was either a re-creation of their original spiritual significance, overriding the meticulous observance of ceremonial details so important in the hereditary transmission of the ceremonies, or success in war sanctioned any ceremonial innovations or uses of known ceremonial details.

None of the reconstruction so far seems to go beyond Boas' statements in his 1897 study, although it brings together conclusions he made at later dates and in other fields. However, it seems possible to expand this reconstruction of the background of the nature of Kwakiutl culture from 1850 to 1920, if some

fairly obvious implications are drawn from the ideas and materials he has given us. In the days when the Kwakiutl consisted of relatively small and isolated groups, warfare may have been important for survival. Perhaps it is not too much to say that the prestige which any group might have would come from its offensive and defensive powers. Under such conditions it would not be surprising if warlike or cannibalistic supernatural beings played an important part as guardian spirits, or if ceremonialism developed about them. The idea of hereditary crests from the north might then have become important because it involved both prestige and a new reason for peaceful affiliation of groups through intermarriage. There would be an increase of the prestigeful intergroup marriages that had always been valued (Boas 1940:368), and enthusiasm for this expansion of peaceful and prestigeful relations could underlie the secularization of supernatural life, the nature of ceremonial life, and even the reworking of oral literature in terms of the idea of crests.

I have attempted here to illustrate Boas' interest in reconstructions that were functionally interconnected with the Kwakiutl culture of the potlatch period. The example could as well have centered upon potlatching, art, or a number of other cultural aspects, since all his reconstructions involve functional interrelations. The task that he did not perform, and that remains to be done, is to draw all of the material together, but the size and consistency of his work will allow this to be done significantly.

With the exceptions of his work in linguistic acculturation (1932), and his willingness to include in text such modern accounts as those of the death of children caused by their throwing gasoline on the fire (1928:148–9) or of prostitution in the city of Victoria (1925:93–94), he was not interested in "acculturation" as we use the term today (primarily the impact of Western European culture on the native culture). When he says "I stressed the necessity of the study of acculturation" (1936:311) and refers to his *Indianische Sagen* of 1895, it is clear that he is using the term in its broadest possible meaning. For his own acculturation interests were in looking back into the history of the Kwakiutl he knew rather than tracing new cultural developments, or the developments he could have recorded in the 44 years between his first Kwakiutl field trip and his last in 1930. His most important syntheses were acculturation studies, in the broad sense he gave the word, and many of the specifically Kwakiutl references explore the relations with other cultures and make inferences about the manner in which Kwakiutl culture reworks materials from outside. An example would be the contrast he drew between the coherence of Bella Coola mythological concepts and the lack of systematization of many of the same concepts among the Kwakiutl (1914 in 1940:470). These synthesizing acculturation studies—*Indianische Sagen* (1895), *Tsimshian Mythology* (1916), *Primitive Art* (1927), *Kwakiutl Culture as Reflected in Mythology* (1935), and so on—are all "symbolic studies."

Other important special Kwakiutl studies are also "symbolic studies": the large linguistic studies, the studies of linguistic acculturation already

mentioned, and "Metaphorical Expression in the Language of the Kwakiutl." In this last, Boas documents such important generalizations as "In the language of the Kwakiutl Indians of Vancouver Island metaphorical expressions referring to unhappy events are of euphemistic character" (1940:232). The study of the "Religious Terminology of the Kwakiutl" (1940:386–392) should also be mentioned as a systematic attempt to collect and order religious terms and concepts. It might also be noted as an example of Boas' thoroughness that in its six pages it contains more than 100 citations from five sources, mostly textual and mythological. Boas' study of Kwakiutl geographical names should also be mentioned here.

The next general point about Boas' understanding of Kwakiutl culture hinges upon a definition of culture, always a difficult matter. Boas' definition of culture seems to have been very strict, for he neither recorded nor caused to be recorded much about informal behavior, as distinct from formal public affairs, myths, family histories, and such surely cultural matters. His desire not to intrude upon the material, not to impose his reactions upon it, is no doubt involved; but anecdotes I have collected about him from informants suggest that something has been lost that, while not clearly to be included as cultural in a strict definition, is possibly of some importance. One anecdote can serve as an illustration. Boas took Daniel Cranmer, his linguistic informant, to Radio City Music Hall in New York City. Cranmer described to me how the show had a "lot of nearly naked women in it" and how "Boas sat back, put his chin in, shook his head and said, 'No good!' "; and how he laughingly answered Boas with "I am enjoying myself." In almost none of Boas' writings or in those of George Hunt is the ribald in Kwakiutl life visible. A minor exception is reported in Boas' eyewitness accounts of the winter dances at Fort Rupert in 1895 (1897:546). Perhaps such things as ribaldry and lustiness, with notes on the kinds of occasions that dependably evoke them, are not "cultural" and it is the great symbolic systems and structures that are truly cultural, but it might be possible to turn Boas' own words against him here. If, as he says, "Ethnology . . . does not deal with the exceptional man; it deals with the masses, and with the characteristic forms of their thought" (1940:314), should it not deal with more informal, messy, and anecdotal material than we have in Boas' Kwakiutl record? There is no question, however, that the formal public reportage and the record of repeatable material, perhaps most particularly linguistic and other symbolic material, surely deals "with the characteristic forms" of the thought of the people and is surely cultural. Language, for example, is cultural; speech, while overwhelmingly cultural, extends beyond some uncertain boundary to the noncultural.

I should like to summarize Boas' understanding of Kwakiutl culture in general terms, for it seems to me that his legacy is not only in the specific analyses he made or the enormous mass of ethnographic detail he collected and recorded, but also in showing things about Kwakiutl culture that require

generalization for all cultures. They are generalizations that no anthropologist can afford to forget, for they are probably true for all cultures, having been proved for Kwakiutl.

Boas saw Kwakiutl culture as a culture:

—significantly related to other cultures historically;

—built to a significant degree of materials from other cultures which were then reworked into Kwakiutl culture in characteristic Kwakiutl ways;

—possessing inconsistencies, conflicts, and incomplete, unsynthesized points of view and world views (1940:446–7);

—consisting of nothing trivial (despite his somewhat narrow construction of the culture concept) in the sense that anything might be regarded as expressive of the psychic processes and interests of the people: recipes, dreams, geographical names, swear words, metaphorical expressions, euphemisms and so on;

—having central themes, even preoccupations, but not having "unity" any more than it has historical isolation and independence (1940:446);

—being so remarkable a human creation in its own right that any judgment of it is irrelevant and the people living it need neither apology nor support; Boas indulges in no panegyrics of the Kwakiutl nor in any other outside judgments, after some naive statements made prior to 1890.

## BOAS' VIEW OF KWAKIUTL CULTURE IN RELATION TO THE CULTURES OF THE NORTHWEST COAST

Just as Boas' view of Kwakiutl culture leads immediately to considerations of the past historical relationships of Kwakiutl with other Northwest Coast cultures, his view of other Northwest Coast cultures leads to generalizations about the special character of Kwakiutl culture. Nothing could demonstrate more tellingly the nature of Boas' Kwakiutl and Northwest Coast work than the fact that special Kwakiutl concerns are placed in their general areal and historical context and non-Kwakiutl concerns are related at some point to Kwakiutl. Generalizations from Kwakiutl culture's own areal, cultural and historical context are therefore always made to illumine Kwakiutl; they are never the comparative generalizations made from a Western European context or from a romantic, rationalistic, political, or other point of view, such as disfigure many ethnological studies.

The Northwest Coast mythological studies form the largest group of comparative investigations and offer the best illustration of how Kwakiutl culture is illumined through comparison. The Kwakiutl are described as rich in story-building and the most independent of the surrounding groups in the character of their stories (1895:340, 346). In contrast to Tsimshian, the Kwakiutl "mental attitude" toward the transformer is one of some respect for his altruism. But their attitude is a mixed one, since they show respect only to the particular transformer whose legend belongs to their own group, while, like the Tsimshian, they show no respect for the transformer in legends belonging to other Kwakiutl groups (1898:413–414). In energy and origi-

nality of story-building " . . . the Kwakiutl have apparently a considerable originality among their neighbors on the North Pacific Coast, because all the numerous social divisions and secret societies of the tribe possess origin tales of the same type; so that a complete list would probably include hundreds of stories more or less strictly built on the same patterns" (1914:481). Since these Kwakiutl origin tales are not harmonized with one another, Boas notes that they have a thoroughly disconnected character and lack of systematization compared to those of the Bella Coola (1914:470).

The most detailed comparison is made with Tsimshian mythology and the summary of his findings is of exceptional interest:

> Setting aside this story [the story of Scab, which has an unusually complex plot for Kwakiutl] and a very few others it may be said that in Kwakiutl stories dealing with human society the interest is mainly sustained by their specific interest in rank and privileges and that without this they are lacking in variety of subject matter and in skill in composition. The general human interest and the imaginative power exhibited by the Tsimshian is much greater.
>
> The animal tales of the two tribes on the other hand have much in common. They are anecdotic in character and dwell especially on voracity and sex as traits that attract the interest of the audience. The Kwakiutl seem to have had a feeling for the incongruity of coarse anecdotes and the dignified character of a culture hero. While among the Tsimshian these two traits are combined in the person of Raven, among the Kwakiutl Mink and Raven are the tricksters while the transformer is an independent human character (1935:190).

The comparative mythological studies illumine Kwakiutl culture at every point. It may be some time before anthropology will develop concepts and methods for handling such facts as the absence of general concern with human interest and motivation in myths and tales, but the material on Kwakiutl culture is ready for such use.

Again, apart from the matter of historical connections, the comparative material on Northwest Coast social organization shows something about the character and qualities of Kwakiutl culture. As Boas says, "It appears to me largely as a psychological question how the highly specialized use of privileges may have been superimposed upon an older simple organization which has a rather wide distribution on the coast," and it is in the contrast with other Northwest Coast tribes that the Kwakiutl are seen as thoroughly "saturated" with the use of privileges (1920:123).

In the comparative materials on art, Boas finds Kwakiutl art less realistic and less crude than Nootkan, although both have similar motives and interpretations (1940:560). Realistic representations are more frequently found among the Kwakiutl than among the people of the north, and among the Kwakiutl "are principally caricatures that are made and exhibited for the purpose of ridiculing a rival" (1927:289). Kwakiutl distortions in painting are described as "if anything, more daring than those of the Haida" (1927: 288), and Kwakiutl artistic developments are regarded as "exuberant" (1927:281).

Boas' many linguistic studies do not generate the same type of comparative statement. Beyond documenting the failure of linguistic and cultural boundaries to coincide, he did not analyze the language-culture relationship within the Wakashan group of Northern Kwakiutl, Southern Kwakiutl, and Nootka. His studies may make this possible in the future.

The comparative view of Kwakiutl culture in relation to other Northwest Coast cultures seems to be based almost entirely upon studies of symbolic cultural materials, particularly those of mythology—probably because the Northwest Coast was an ecological and cultural area which shared fundamental technological and socio-economic features. Boas even considered that "the material culture of the fishing tribes of the coast of northeastern Asia, of northwest America, and of the Arctic coast of America . . . [were] . . . so much alike that the assumption of an old unity of this culture seems justifiable" (1940:338). It would follow that his work in clarifying cultural relationships within the Northwest Coast area, and the relationships of that area to the Old World, would necessarily deal largely with symbolic materials, since the most significant variation would be found at the nonmaterial pole.

## CONCLUSIONS: BOAS AND THE UNDERSTANDING OF THE KWAKIUTL

Boas' Kwakiutl work is the product of a consistent, monumental, and unfinished plan motivated by the desire to understand a people. Since he saw this desire as understanding the "mental life" of a people as reflected in their culture, it is entirely consistent that he tried to trace every historical relationship so as to isolate what was peculiar to Kwakiutl and that he placed special emphasis upon and special trust in collecting and studying symbolic materials. The latter can seemingly be obtained and used with the smallest chance of distortion, of intruding an alien mental life and alien interest upon the culture. The power of Boas' motivation to understand the Kwakiutl must have been extraordinary, for the amount of work accomplished, its meticulousness, and his apparent willingness to follow its courses even when they led through jungles of data, would otherwise be incredible.

Contemporary anthropologists have been able to adjust to working in acculturation situations where the non-European culture is sometimes almost obscured by the European culture, because Boas and others did record a culture in the earlier stages of contact. It would now be quaint in many instances for an anthropologist to make the old archivist appeal which Boas made successfully to Daniel Cranmer: "In a while your language will exist only in these books." But although the understanding of the Kwakiutl would have benefited by the addition of more European "contact-acculturation" materials in Boas' work, it would merely be a matter of addition. It is the earlier, more purely Kwakiutl materials that are now irrecoverable and that are required for the understanding of the present-day people and of the historical changes during the past four decades.

Knud Rasmussen's statement, after 18 years spent with the Eskimo, was "that one is never done exploring a people." Boas would certainly agree after

his more than 50 years of Kwakiutl and Northwest Coast work, for his own statements and his unpublished works show that his plan was still incomplete. His *Kwakiutl Ethnography* will bring the plan nearer to completion, but I doubt whether it will fully realize his goal of synthesizing Kwakiutl culture. Although Boas' goal of understanding a people is indispensable for anthropology, it may be impossible to attain. Boas' criticisms of attempts to use his Kwakiutl work to derive synthesized understandings seem justified, for none of these attempts uses all the Kwakiutl material, avoids oversimplification, or solves the problems of relating the culture's symbolic systems to each other and to the whole life of the people (1940:446–449; 1936:267; 1939:684–685).

Since Boas, no new anthropological aim has replaced his aim of understanding people through understanding their culture. There seem to be no new and powerful concepts that apply to many of the problems raised by Boas' Kwakiutl work, especially his work with the symbolic areas and aspects of culture. What does it mean that in Kwakiutl culture there are euphemisms for unhappy events, that stories in which human beings figure have the acquisition of crests as almost the only motive and plot, that the art is preoccupied with animal forms? What does it mean that the content and structures of the symbolic worlds of myth, language, and art show various special features and emphases? And what do these things mean when taken altogether as Kwakiutl culture? What does it mean that a culture like Kwakiutl has both an independent existence and almost no unique content when compared with neighboring cultures? These are fundamental questions in anthropology and the understanding of the Kwakiutl will not be complete without answers to them. While the achievement of that goal must await improvement of the concepts and methods of anthropology, Boas' Kwakiutl materials are ready to work with; an important part of his legacy is that the understanding of the Kwakiutl is already far advanced.

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# *Boas' Contributions to Knowledge of Human Growth and Form*

J. M. TANNER

TRADITION has it that a railway train bore Boas into the field of human growth. True or apocryphal, the story is that Boas, en route to the Cleveland meeting of the American Association for the Advancement of Science in 1888, became engaged in conversation with the man sitting next to him. As the train drew into Cleveland the man was revealed as G. Stanley Hall, a redoubtable figure in contemporary American academic life, who had just become the first President, and Professor of Psychology and Pedagogy, at a brand-new University named Clark at Worcester, Massachusetts. Stanley Hall is now chiefly remembered for his remarkably polysyllabic and mid-Victorian book on Adolescence, which, though published in 1904, is full of phrases like "the secret vice," "enfeebled heredity," and "the evolution of the soul." But he was a firm champion of the view that psychology must be based on fundamental biological concepts and that the first thing in education was to understand the manner of growth of the body and brain of the child. In many ways he was a far-sighted man; it was he who as early as 1909 invited Freud to deliver his first lectures on psychoanalysis given outside central Europe. He had a talent, too, for spotting men of exceptional promise, and he ended that railway journey by asking Boas to join him as head of a division of anthropology in his own department of psychology. Thus, Boas joined the young but later famous physicist A. A. Michelson and the biologist Henry H. Donaldson on Clark's somewhat impoverished but certainly enthusiastic campus.

This was in 1888 and Boas was thirty. Despite his already wide interests and growing reputation in other branches of anthropology, Boas embarked on the study of human growth. Doubtless his interest was quickened by Stanley Hall, and probably by Henry Bowditch at Harvard University nearby; but subsequent events showed it needed little stimulation. Boas' first paper on growth appeared in 1892, his last in 1941. He was largely responsible for the discovery that some individuals are throughout their childhood more advanced on the road to maturity than others, and for the introduction of the concept of physiological or developmental age. It was his studies which established growth and development securely as an item in the practice and teaching of physical anthropology in America. Indeed, the first Ph.D. ever given in anthropology by an American university was to his student and successor A. F. Chamberlain in 1892 for a study on the heights and weights of Worcester children.

## THE BACKGROUND TO BOAS' STUDIES ON GROWTH

The two outstanding contributors to the field of human growth when Boas entered it were Charles Roberts of Uxbridge, an English medical practitioner, and Henry Bowditch of Harvard, the first American professor of physiology. These two worked at much the same time and independently, but exchanged unpublished data across the ocean. They approached their subject from different points of view. Roberts had a strong interest in public health matters and acted for a time as assistant factory commissioner charged with the task of seeing whether child labor in the Lancashire factories produced ill effects on development, and of devising minimum standards of growth and fitness for permitting a child to work. He followed in the direct line of Cowell, who in 1833 published what was probably the first survey of heights and weights of children ever made. Bowditch, on the other hand, was particularly struck by the details of the growth curves themselves as seen in some private longitudinal data on a couple of dozen children evidently collected by a Galtonian spirit in Boston. It was the fact that girls were larger than boys for about two years during their growth that especially intrigued Bowditch; that, and the discrepancy in this respect between his Boston curves and the Belgian figures of Quetelet (1870).

Roberts (1874, 1876, 1878) and Bowditch (1877, 1879, 1891) came to very much the same conclusions. Each discovered for himself the existence of the adolescent growth spurt and its earlier occurrence in girls than in boys. Each remarked that growth in height ceased earlier in girls than boys. Both studied—often on each other's data—the effect of poverty and of parental nationality on growth. Roberts divided his 16,000-odd children into those belonging to the "labouring and non-labouring classes" and showed the superiority of the latter in height and weight at all ages. Bowditch compared Boston children of American-born parents with those of Irish-born parents and showed a difference in favor of the former. He also compared Roberts' British children from private schools and universities with their Boston equivalent and found the Americans larger, concluding from this that, although poverty has its effects, there are in addition "influences prevailing in this community other than those connected with the comfort or misery of existence which give to a growing boy a greater height and weight than are attained by an English boy of the same age" (Bowditch 1877). Both workers realized, too, that the lower socio-economic groups had a higher weight for given height than the upper groups, a fact subsequently forgotten till very recently (see Healy 1952; Tanner 1955).

## TEMPO OF GROWTH

Neither Roberts nor Bowditch, however, stumbled on the idea of one child being more advanced in his growth than another. This was Boas' great and fundamental discovery in human growth—a discovery summed up in his own phrase, *tempo of growth.*

He approached this discovery in a very characteristic way. Boas was an

exact contemporary of Karl Pearson, and for many years was in the forefront of those developing the new techniques of biometry. Undoubtedly during the period 1880–1910 he brought to his problems a greater degree of statistical knowledge than practically anyone else concerned with human biology in America or Continental Europe. To begin with he noticed (1892) that the distributions of heights at each year of age in Bowditch's 1877 report were not quite symmetrical about the average values, and that there were differences between the arithmetic means and the modes (most biometrists of this time, incidentally, called the arithmetic mean the "average" and called the mode the "mean," a terminology confusing at first to the occasional modern reader of their papers). Believing Quetelet, perhaps, and trusting in the universality of the beloved "law of errors" he writes that in adults the distribution of height and (quite erroneously) of weight is what we now call Gaussian. Why then was this not so in childhood?

Here follows a remarkable piece of reasoning, unfortunately expressed in obscure English followed by almost impenetrable algebra. In retrospect the essence of this thought is clear, however: at any given year of age we are to assume that the physiological status of the children is normally distributed around an average value. This status is simply a theoretical conception of acceleration-retardation in growth and has no operational definition. Its distribution at any age is assumed to be Gaussian, since Boas at that time ascribed its variability to a multitude of "accidental causes" of environmental origin. Boas then points out the effect illustrated below in Figure 1. When the average rate of growth is not changing, the individuals advanced in physiological status have gained as much above average in height, say, as those retarded in physiological status have gained below average; the two balance and the curve of height remains symmetrical, like the curve of physiological status. But if the mean rate of growth is increasing, as occurs at puberty, then for a given degree of physiological advancement a person has actually gained more height than that corresponding to a similar quantity of retardation. Thus the average is elevated, though the mode stays as before. The opposite occurs when growth rate is decreasing. In a word, the effect of a changing growth rate is to alter the symmetrical curve of heights, occasioned by a Gaussian distribution of "physiological status" to a curve with a long tail to the right

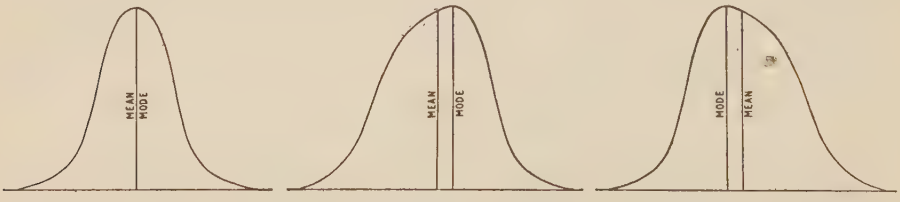


Fig. 1. Skewness produced in yearly frequency distributions of anthropometric measurements by changes in mean rate of growth during the year. Left: (a) rate of growth nil or constant: no skew. Center (b): rate of growth decreasing: negative skew. Right (c): rate of growth increasing: positive skew.

TABLE 1. MEASURES OF SKEWNESS AND KURTOSIS OF ANNUAL DISTRIBUTIONS OF HEIGHT FOR BOYS

| Age last birthday | Number | $g_1 \pm$S.E. | $g_2 \pm$S.E. | Mean rate of growth at 5.5, 6.5, 7.5 etc. (cm/yr) |
|---|---|---|---|---|
| 5 | 905 | −1.19 ±0.08 | +0.90 ±0.16 | 5.9 |
| 6 | 1131 | +0.13 ±0.07 | +0.10 ±0.15 | 6.3 |
| 7 | 1221 | +0.03 ±0.07 | +0.19 ±0.14 | 5.9 |
| 8 | 929 | +0.10 ±0.08 | +0.12 ±0.16 | 5.4 |
| 9 | 1023 | +0.13 ±0.08 | +0.22 ±0.15 | 4.9 |
| 10 | 901 | +0.11 ±0.08 | −0.29 ±0.16 | 4.9 |
| 11 | 1186 | −0.06 ±0.07 | +1.16*±0.14 | 4.9 |
| 12 | 1062 | +0.27*±0.08 | +0.15 ±0.15 | 5.3 |
| 13 | 1017 | +0.17*±0.08 | −0.13 ±0.16 | 6.6 |
| 14 | 1028 | −0.61*±0.08 | −0.31*±0.15 | 6.4 |
| 15 | 373 | −0.45*±0.13 | −0.04 ±0.25 | 4.8 |
| 16 | 127 | −0.57*±0.22 | +0.17 ±0.43 | ca.2.0 |

* Significantly different from zero at 5 percent level.

Calculated from London County Council cross-sectional data (Scott 1955). Age 5 signifies 5.0 to 5.95, etc.

and a threshold cut-off on the left when growth rate is increasing, and the opposite when it is decreasing. Boas actually suggested a formula for correcting for this effect, but recognized later (1906) that it made too many assumptions. The effect in any case is quantitatively small.

What is so difficult to believe is that Boas really came to discover the factor of acceleration/retardation in growth in this way. It seems far more likely that the idea came to him intuitively and was consolidated by his experience of measuring and observing children during the year 1891–2 when he conducted a short-lived longitudinal study in Worcester. Statistical consequences would follow immediately. Immediately, that is, for Boas: for the astonishing thing is that this particular effect has gone unnoticed and unremarked right from 1892 to the present year. Usually the Gaussian distribution of height at all ages is taken—erroneously—for granted. However, early this year M. J. R. Healy and the writer examined more closely the London County Council statistics and found they produced the figures for skewness and kurtosis given in Table 1. Even then the explanation of the figures failed to dawn upon us until reading Boas 1892 for the purpose of this essay. Nobody ever accused Boas of being either superficial or short-sighted, and one can see why.

Boas' statistical acumen led him to note other "distortional" effects in the biometry of growth curves. In Bowditch's cross-sectional data the variability of the distributions of height and weight increased greatly at the time of

adolescence, and Boas recognized that this also was, as he put it, "a purely statistical phenomenon." It was created by adding together figures for individuals of different degrees of acceleration and retardation. In 1892 he did not yet realize the full significance of this effect or clearly separate it from the skewing effect, because he was not yet thinking in terms of velocity curves of growth. Thus the explanation and illustration given below carries us beyond his position in 1892 to the threshold of his second period of papers upon growth. By 1930 he had fully absorbed the implications of his discovery and it was he who then initiated the use of the "maximum growth age" or "time of peak velocity," his second great contribution to the methodology and theory of human growth.

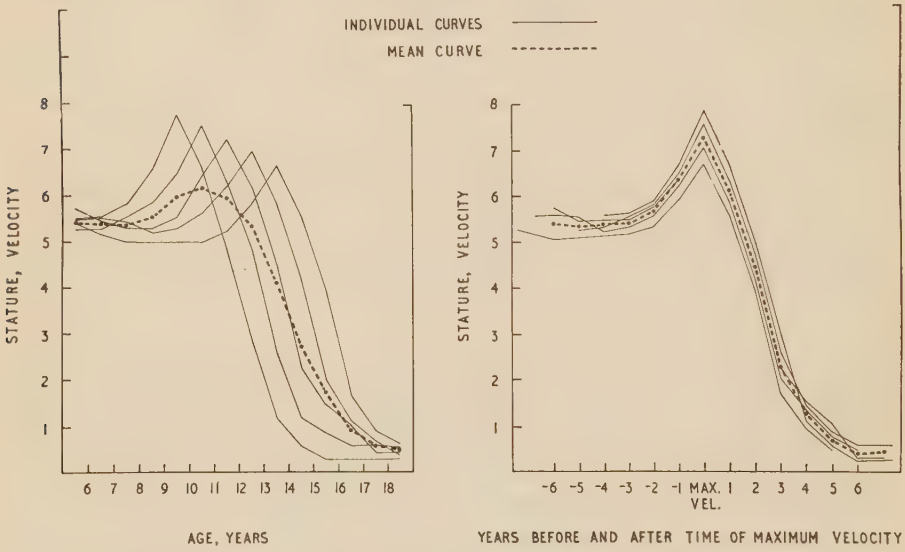


Fig. 2. Tempo effect on mass velocity curves. Relation between individual and mean velocities during the adolescent spurt. In Fig. 2a, left, the height curves are plotted against chronological age; in Fig. 2b, right, they are plotted according to their time of maximum velocity. (From Tanner 1955.)

This effect, which we may well name, by using his favorite word, "the tempo effect" on mass velocity curves is simply explained thus: the time at which the adolescent spurt begins varies from child to child; probably its distribution is roughly Gaussian. Figure 2a shows a series of individual velocity curves from 5 to 18 years, each individual starting his spurt at a different time. The mean of these curves, obtained by treating the values cross-sectionally and simply taking the average value at each age, is shown by the heavy interrupted line. It is obvious that this line characterizes the average velocity curve very poorly; in fact, it travesties it, smoothing out the adolescent spurt and spreading it along the time axis. The realization of this effect was undoubtedly one of the reasons for Boas' insistence on the importance of

individual longitudinal studies which would give growth curves free of this artifact. However, it also was ignored by nearly all students of growth until revived and clarified by Boas himself in 1930, and restated by Davenport in 1931 and Shuttleworth in 1937.

The effect has, of course, the same origin as the skewing effect first noticed by Boas: both are due to differences between children in tempo of growth. But we can distinguish them by remarking that an increase in the variability of the hypothetical "physiological age" simply brings about an increased variance of values at the year concerned. Such an increase in scatter of the tempo is presumed to occur at adolescence, as Boas soon saw. But it would not alone bring about skewing of the distribution curve. For this the added factor of a change of mean growth rate is necessary. This of course does also occur for most measurements at puberty, so that both of these tempo effects as a rule appear then. However, it is an interesting comment on the biometrical atmosphere of the 1890's that the very minor skewing effect and not the major variance increase was the one which caught Boas' eye. The belief in the universal applicability of the "law of errors" was so strong that a small departure from it appeared more obvious than a large but regular increase in the variance itself.

## THE FIRST AMERICAN LONGITUDINAL GROWTH STUDY

The importance of longitudinal growth studies was obvious enough to Roberts, who wrote in 1874: "This subject would be best illustrated by a series of successive annual measurements of the same individuals (especially of girls in whom the marks of puberty are more easily recognized), a labour however, which could only be effectively carried out by a large number of observers." Boas' recognition of the tempo differences made such a study imperative. "During the past years," he wrote (1892), "a vast number of observations referring to the growth of children have been accumulated. The method of treating the results of such observations has been largely a comparison of averages and of the frequency of occurrences of cases between certain limits. . . . The question arises in how far the results have a physiological meaning and in how far they are purely statistical phenomena. It is generally assumed that the figures express physiological facts. Serious objections, however, may be raised against this view. In almost all cases . . . the observations have been taken only once on a great number of individuals, not repeatedly through a long number of years on the same individuals. . . . A knowledge of the actual anthropometric conditions of children at a given age . . . does not allow us, or at least allows us only in a very imperfect way, to draw inferences of physiological value." Additionally, he notes that growth curves founded on cross-sectional data necessarily take no account of any differential mortality during childhood and suggests that "individuals far remote from the average" (shades of Quetelet's homme moyen again!) probably have a higher mortality.

Accordingly, in May 1891 Boas began a long-term longitudinal study of Worcester school children. In addition to height and weight, sitting height,

forearm length, hand breadth and head length and breadth were taken, and later reported by his assistant, G. M. West (1894) and Boas himself (Boas and Wissler 1906). The study was ill-fated however. Just over a year after it started Boas resigned from Clark University, partly because he was offered a job under F. W. Putnam at the Chicago Columbian Exposition, and partly, it is said, because of a disagreement with Stanley Hall, a disagreement which did not, however, prevent Hall from writing of Boas a dozen years later as "our best American authority on the treatment of these [growth] statistics."

Only one year's growth had been completed by his subjects, but when he wrote up his results in 1897 Boas used his scanty material to excellent effect. The experience of actually handling his own longitudinal data seems to have clarified considerably his conceptions and in this paper one feels he has really got a grip on the acceleration/retardation problem. He gives means and standard deviations of the yearly increments, and comments that "young children grow more uniformly than older children. The increase in variability [of growth rate] is very great during the years of adolescence . . . this increase must be considered due to the effects of retardation and acceleration." He then divides his data into two halves, one containing all the children who are shorter than average for their age and the other all the children who are taller. He found the shorter children grew less than the taller in the years before adolescence. But during adolescence the shorter grew more; that is, they continued to grow at a time when the taller were stopping. He thus demonstrated for the first time the relative independence of final adult height and the speed with which it is reached. He overplayed his hand, perhaps, in concluding that "small children are throughout their period of growth retarded in development, and smallness at any given period as compared to the average must in most cases be interpreted as due to slowness of development." He probably had a special reason for doing this, and perhaps a special reason, too, for concluding (however correctly) that "the differences in development between social classes are to a great extent results of acceleration and retardation of growth."

## PHYSICAL AND MENTAL GROWTH

The special reason was a battle he was fighting over the interpretation of some results published by W. Townsend Porter of St. Louis. Porter (1893) had pointed out that at any given age the school pupils in the higher grades were taller and heavier than their coevals in the lower grades. Porter talked of physical growth being the basis of mental growth, and referred to the pupils as bright and dull respectively. He also promulgated the opinion that growth was regular and therefore the "typical boy" would stay at the same percentile position relative to his group throught the growing period. To all these things Boas (1895a) lodged strong objections. First, with perhaps understandable tartness, he wrote, "Dr. Porter has shown that mental and physical growth are correlated, or depend upon common causes; not that mental development depends upon physical growth." Second, he said that the words "advanced" and

"retarded" should be used instead of "bright" and "dull" since the effect was probably due to differences in speed of development. Third, he argued that the "typical boy" would scarcely have the exactly average rate of development all his childhood and therefore would not stay at the same percentile; only a very rare boy indeed would do so.

This last objection seems largely a matter of words, depending on whether by typical one means simply "modal" or a more abstract conception similar to the ideal man developed out of the "homme moyen" by the older constitutional anthropologists. But the objection that bright and dull should be replaced by advanced and retarded is more substantial and still carries a lesson for us now. Porter and Boas were writing a decade and more before Binet published his work, yet intelligence tests of all sorts still are constructed and interpreted on that basis of bright and dull, and allow nothing to the factors of advancement and retardation. Neither do age-bound school examinations, such as the Eleven Plus in England, pay any heed to the tempo differences. Indeed, most modern educators seem to be further behind than Boas and Stanley Hall in this matter. Perhaps this is due to the lack, until recently, of careful examination of longitudinal records of intelligence scores. In the modern work of Nancy Bayley (1956) and Sontag and his colleagues (1958) indications of tempo of growth in mental ability seem to be clearly present.

Porter's factual observations on the relation of size to school grades have been repeatedly confirmed over the years (for example, by Baldwin in 1914) and extended to show a relation between size and I.Q. (see Tanner 1955:142, Baldwin 1921:235). Boas himself reported similar statistics to Porter's in 1906 for his Worcester children. He gave a table beginning thus:

| | Grades: | 1 | 2 | 3 | 4 | . . . |
|---|---|---|---|---|---|---|
| Ages: | 7 | 6.9 | 7.8 | 7.9 | | |
| | 8 | 7.7 | 8.3 | 8.9 | 9.5 | |
| | 9 | | 9.0 | 9.4 | 9.7 | |
| | . | | | | | |
| | . | | | | | |
| | . | | | | | |

The table initially was one of *height* for given age and school grade; then for the height figure was substituted the *age* at which that height was average. Thus among pupils 8 years old, those in the first grade had a "height age" of 7.7 years, those in the second 8.3 years, in the third 8.9 years, and so on. Boas attributed these results chiefly to variation in tempo of growth rather than in final ability. He evidently felt that he was championing the cause of the late developer, and he never lost his interest in the subject.

In 1941 his last paper on growth returned to the theme, now fortified with intelligence tests as a measure of ability, and skeletal age as a measure of

tempo of bodily growth. His data, never very fully reported, show a significant regression of I.Q. both on stature and skeletal development for a given chronological age. "Retarded children," he wrote, "have a lower intelligence quotient than those accelerated. This corroborates the inference drawn by me from Porter's early observations. . . . The close correlation between anatomical and psychological traits in childhood must be interpreted as due to the influence of the tempo of physiological development over the body and its functions." Perhaps he overdoes it with the adjective "close"; and he seems not to have encountered the growing volume of data suggesting a relation between I.Q. and stature in normal adults (see e.g. Schreider 1956). But how many scientists could write that "corroborates the inference" in respect of work done fifty years earlier?

## STANDARDS OF HEIGHT AND WEIGHT FOR AMERICAN CHILDREN

Besides starting what was to have been the first substantial longitudinal growth study, Boas produced also the first standards for height and weight for American children. "In 1891, when active preparations for the World's Columbian Exposition were being made, Professor F. W. Putnam . . . placed me in charge of the section of Physical Anthropology. . . . We agreed upon a plan to represent as fully as possible the growth and the development of American children" (1898). School authorities were approached and those in Oakland, California, and in Toronto agreed to contribute figures to add to those already collected by Bowditch in Boston, Peckham in Milwaukee, Boas in Worcester, and Porter in St. Louis. A total of nearly 90,000 children between five and eighteen years old was involved. Boas pooled the results from these six cities to produce the composite American standards, remarking that, though he would have liked to weight each series in accordance with the percentage of the total American population it sampled, the lack of accurate census data prevented his doing so.

The standards for height were published in a Report of the Commissioner of Education in 1898. As one might expect, the work-up is a model for all to follow; and as one might depressingly fear, it held at least one point which subsequent authors for sixty years ignored entirely. Boas gives the mean height for each sex at each half-year from $5\frac{1}{2}$ to $18\frac{1}{2}$ and then corrects this figure by linear interpolation to the exact half-year point (since the mean age of his 5-year-olds, for example, was a little over 5.50). He then gives not only the standard deviation calculated from all the values each year, but the corrected standard deviation, in which the "instantaneous" standard deviation at exactly age 5.50, and so on, is presented.

This correction for the artifact due to having in cross-sectional studies data coarsely grouped in yearly intervals seems to have escaped all subsequent workers till 1958, despite repetition of its importance by Boas in a review of Woodbury's standards in 1922. Woodbury (1921) at that time gave standards for height and weight based on 172,000 American children "selected on the basis of reliability of the data from among two million cards received as a

result of the Children's Year campaign." The statistical work-up was poor and Boas had every reason to feel nettled that his thoughts of twenty and thirty years before had been so completely ignored. Yet no trace of annoyance shows through the fairly severe statistical criticism of the review. Boas does not even quote his earlier articles; he simply explains over again. "Another error in these calculations is introduced by the neglect to consider the annual growth. This effect is considerable, particularly when growth is rapid. The variability at a given moment may be called $\sigma^2$. When we call the annual rate of growth $d$, we have a total variability of $\sigma^2+1/12\ d^2$." Consequently, to obtain the "instantaneous variance" $1/12\ d^2$ must be subtracted from the grouped variance.

The first standards subsequent to Boas' in which cognizance of this effect was taken seem to have been those of the writer, produced in 1958 (Tanner 1958) in association with M. J. R. Healy, who redescribed the corrections in somewhat greater detail (Healy 1959). This was once more before we encountered Boas' paper. The reader may imagine that by this time we were prepared to find all our work lying ignored in Boas' papers of sixty years ago!

## END OF FIRST GROWTH PERIOD

Boas left the Chicago Exposition in 1894, and in 1896 settled in New York, at the American Museum and Columbia University, where he was to spend the rest of his life. We may regard this move as bringing to a close his first period of the study of growth. His interest had been increasingly engaged by the problems of the effect of heredity and environment on body form, and in 1908 he began his famous studies on immigrants. Until 1930, when he embarked on a second period of the study of human growth curves, his interest in children's measurements relates rather to their family relationships than to the process of growth itself. During the first years of the century however a further Report of the Commissioner of Education was issued (Boas and Wissler 1906) and a paper (1912a) assimilating to his thoughts the new methods for measuring developmental age then being developed.

The 1906 paper, with Wissler, is chiefly remarkable for presenting what are probably the first correlation coefficients calculated upon human growth data. Boas, as we have remarked already, was in the forefront of the biometrical advance; all his life he taught and encouraged anthropologists in the use of statistical methods. As early as 1894(a) he wrote a paper, expository in character and placed in the American Anthropologist, describing the correlations between two anthropometric measurements. Galton (whom Boas does not mention in this article, but whose work must certainly have inspired it) had only introduced the correlation coefficient in December 1888, and the book that brought Karl Pearson to him, *Natural Inheritance*, appeared in 1889. Pearson's method for calculating the correlation coefficient, which replaced Galton's, was published in 1896. So Boas came to the biometric fold as early as the great K. P. himself. In this 1894 paper Boas presents no coefficients of correlation, but gives diagrams of the regression of two anthropo-

metric measurements, each on the other, in Sioux and Crow Indians. He remarks that the greater closeness of the two lines for stature and span than those for head length and head breadth indicates that the former pair of variables is the more closely associated. He also understood clearly the reduction in variance of one measurement when standardized for a fixed value of a correlated measurement. He remarked that the discovery of ultimate causes of correlations was a futile endeavor and wrote, nevertheless, that "the results of anthropometric statistics are a means of describing in exact terms a certain variety [of man] and its variability . . . it is clear that a *biometric* [his italics] method would undoubtedly open new ways of attacking problems of variation."

The Worcester longitudinal data are used for the correlational analysis of 1906, supplemented by new data from the files of the Newark Academy, a private school near New York. Boas calculates the correlations of stature and other measurements at year $t$ with their respective increments from $t$ to $t+1$. He demonstrates in these terms what he had really already shown by his division into short and tall groups in the 1897 analysis, that "the expected results [occurred], a maximum of positive correlations during the periods of most rapid growth and a sudden drop to negative correlations when growth is nearly completed." For the Newark Academy data, which encompassed longitudinal series of 70 boys from 12 to 17 years, he calculated the correlations between the increment $t$, $t+1$ and the increment $t+1$, $t+2$, and so on, showing them to be positive till 14–15 and negative thereafter.

Boas, with his unbounded regard for scientific integrity and the ethics of research, made a practice of publishing all his raw data whenever possible, so that others also could use them to further knowledge. The 1906 paper starts this habit, and the end of it consists of the full records of the Worcester study, giving the individual measurements of the children, their school grade, birth order, sibling number, and so forth.

It was just about this time that Rotch (1910) and Pryor (1905) demonstrated that children of the same chronological age showed considerable differences in the degree of ossification of the bones of the hand, and that girls were ahead of boys in their ossification at all ages. It is not now clear whether these researches into developmental age were stimulated either personally by Boas or through contact with his ideas on tempo of growth, but it seems very likely. The other advance in developmental age measurement, that made by Crampton, was certainly stimulated, though not actually begun, by Boas himself. Crampton was a school physician in New York who felt the need of some more realistic criterion than chronological age for classifying his adolescent school boys for fitness to take part in athletic programs and such. About 1901, under the influence of Boas and of Stanley Hall, he began studying the growth of pubic hair as providing such a criterion, and in 1908 published an exceedingly far-sighted paper, reprinted in 1944. In this he described a classification by the stages of absence, appearance, and full development of the pubic hair. In the same year he actually joined Boas in his study of immigrants

in New York, since Boas wanted some measure of acceleration/retardation on the children.

Boas welcomed these advances and additions to his basic conception and in his 1912(a) article wrote, "A study of the eruption of the teeth which I made a number of years ago, and the more recent interesting investigations by Rotch and Pryor on the ossification of the carpus, show that the difference in physiological development between the two sexes begins at a very early time and that in the fifth year it has already reached a value of more than a year and a half. . . . The condition of the bones . . . gives us a better insight into the physiological development of the individual than his actual chronological age, and may therefore be advantageously used for the regulation of child labor and school entrance, as Rotch and Crampton advocate." Later, however, he had some qualifications to add. In his summary of his growth work appearing in *Race, Language and Culture* (1940a), he wrote, "There are however differences in the speed of development of various organs. This is brought out most clearly by a comparison of the dates of eruption of [deciduous] teeth of boys and girls. While in all other traits girls of a given age are much more mature than boys of the same age, there are very slight differences only in the eruption of the [deciduous] teeth, proof that they are subject to influences different from those acting upon the skeleton. It is not admissible to assume with Crampton that physiological development is equal to physiological age."

In the same (1912a) paper he presents a thought which stayed with him and recurs in several of his papers, namely that the variability of the age of occurrence of physiological events gets continuously greater throughout the growing period and probably throughout the whole of the life span. He backs this conception by giving a table of the variances in time of gestation, eruption of first incisors and other teeth, occurrence of menarche, menopause, and death from arterial disease. The conclusion he draws is that this variability is not "accidental" but of a physiological nature. Though this would appear very obvious to us now, Boas at first seemed to think, though never very explicitly, that differences in tempo were due to causes following the "law of errors." By this time he had realized such was not the case, and later he was actually to study the inheritance of tempo of growth.

## SECOND GROWTH PERIOD, 1930–1942

Apart from a few talks to associations and the 1922 review of Woodbury's standards, Boas published little on growth itself from 1912 to 1930. During this time he was concerned more with biometrical genetics, to a consideration of which we shall turn shortly.

But in 1930, at the age of 72, he produced the first of two papers embodying a second major contribution to the understanding of human growth. A number of individual growth curves had accumulated in the literature since 1906. In particular there was the work of Baldwin, an educational psychologist who reported in 1914 the second longitudinal growth data systematically collected

in the United States, and became in 1917 the first director of the Iowa Child Welfare Research Station. His monograph of 1921, justly famous for its annotated bibliography of studies of growth in all countries up to that time, contains a rather half-hearted analysis of the growth of 60 boys and 60 girls followed in the Horace Mann School of Teachers College, Columbia University, over various periods from age 7 to 17. Baldwin had data on age at menarche in the girls and did indeed conclude that growth in stature ceases soon after the menarche. But despite drawing probably the first graph of velocity of growth in height for different age-at-menarche groups, he missed most of the points that Boas later seized upon.

Boas, we may presume, had pondered over these longitudinal data and come to realize that they held more possibilities of analysis than their authors had exploited. He also realized that his own correlational methods of 1906, useful indeed for analyzing a longitudinal series consisting of only two values a year apart, were insufficient to extract more than a fraction of the information contained in the growth curves of individuals followed over many years. Their inadequacy was felt especially in dealing with events of adolescence. Boas wrote that he "demonstrated a number of years ago [in the 1906 paper] that . . . the actual individual growth rate must show a much more decided decline during childhood and a much more decided increase during adolescence, and that at the same time the period of increased rapidity of growth during adolescence must be much shorter than would appear from the generalized curve" (1930). He now wished to study the details of these individual curves more closely. He wished to verify that children who were taller at age eight, say, had indeed early adolescent spurts, as his old Worcester data had led him to believe; and he wished, in particular, to see if final adult height was related in any way to the time of occurrence of the spurt, or to its rapidity or intensity. Initially he used the same longitudinal records as in 1906, from boys at Newark Academy, but this time supplemented where possible by records of their adult stature. For the later paper, chiefly published in 1932, but with continuations in 1933(a) and, less importantly, in 1935(a), he added similar longitudinal though retrospective material from the Horace Mann School, the Ethical Culture Schools, and the City College of New York.

Because "the fundamental questions [at issue] are not well expressed by the coefficient of correlation" (1930) Boas used graphical methods, subdividing his subjects into groups on the basis of height at 14, or age at maximum velocity, or age at menarche, and then plotting the growth of the means of each of the groups. The method, in Boas' hands, was a powerful and productive one. It inspired Shuttleworth's more detailed but essentially very similar studies on the more plentiful Harvard Growth Study data some five years later (Shuttleworth 1937, 1938, 1939). Nowadays curve fitting to the individuals' data (see e.g. Deming 1957) would be the appropriate, and indeed the equivalent approach. But in 1930 electronic computers were not available and efficient methods of curve fitting were in their infancy; also Boas liked to handle his data himself and not leave them to a multitude of assistants working desk machines.

The essence of his findings is shown in Figures 3, 4 and 5, redrawn from illustrations in the 1930 and 1932 papers. In Figure 3 (1930: Fig. 4) the distance curves for stature are given for five groups of boys, the grouping being by age at maximum velocity of growth. This is the first introduction of this method of classification. Two effects are clearly demonstrated: that on average those who had an early adolescent spurt were already taller by age 11, but

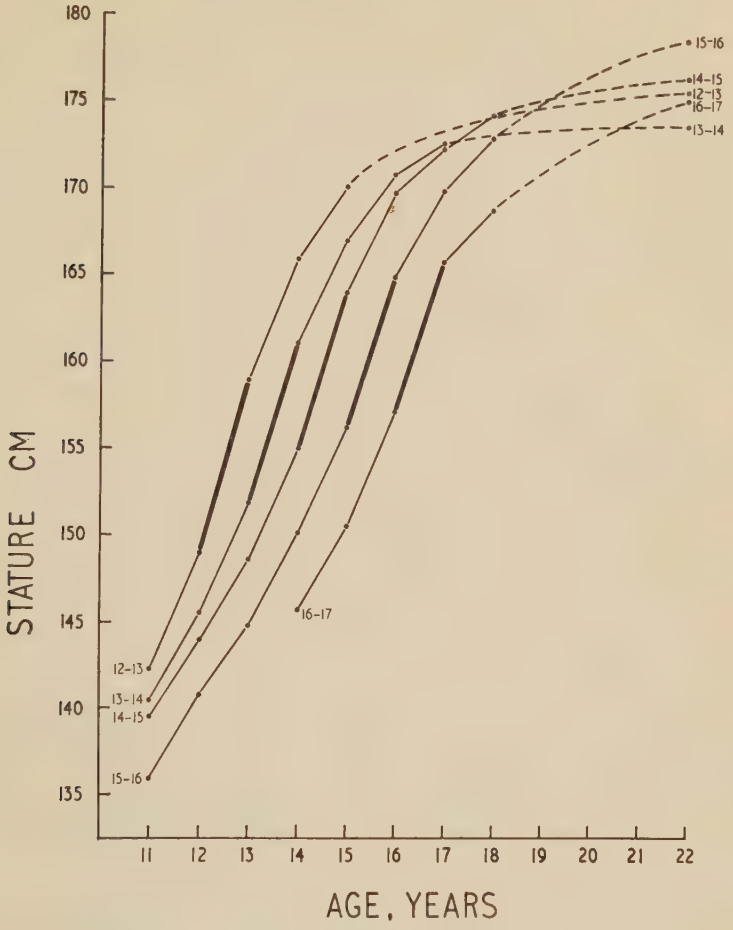


Fig. 3. Mean stature of Newark Academy boys having maximum rate of growth at ages 12–13, 13–14, etc. (Boas 1930: Fig. 4 redrawn.)

not necessarily taller when adult stature was achieved. Having regard to the relatively small numbers in the groups and the irregular positioning of the adult values, Boas wrote "we may well assume that there is no relation between the time when the greatest rapidity of growth sets in and the stature finally attained." He typically added: "This conclusion is subject to revision when more material will be available." He noted—evidently with satisfaction—that the standard deviation rose much less at adolescence in each of his

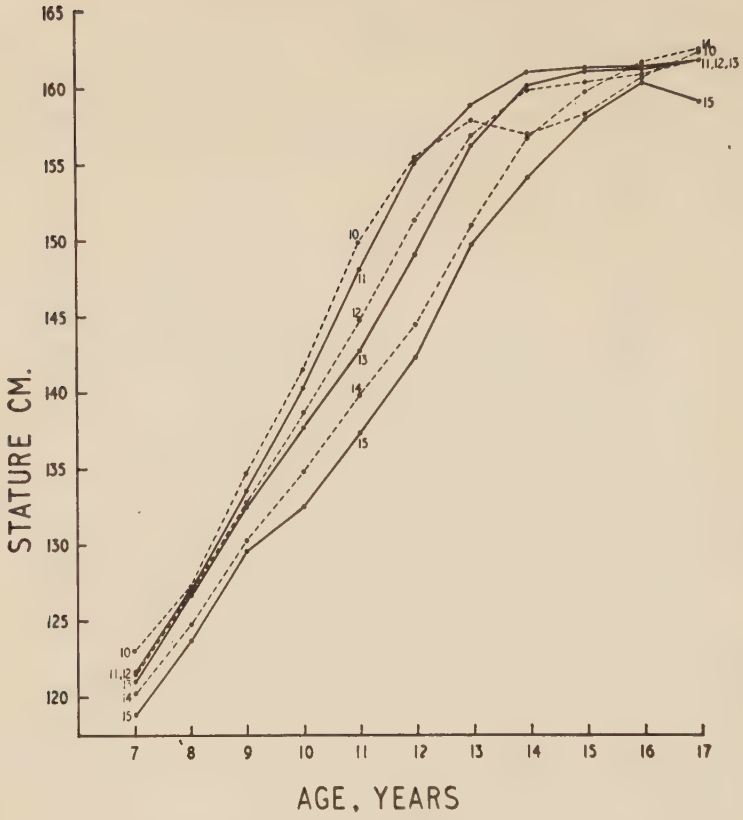


Fig. 4. Mean stature of all Horace Mann School girls having menarche at ages 10–, 11–, 12–, 13–, 14–, 15–. (Boas 1932: Fig. 2 redrawn.)

groups than it did in the whole pooled series. This bore out his contention that the variance increase was due to the spurt occurring at different times in different individuals. He remarked that the effect had not entirely disappeared, however, and that the increase was more marked in the later- than in the earlier-maturing groups. He also gives a table amplifying his 1892 description of the skewing effect on distance curves: here he points out that the distribution of increments even within his groups, though symmetrical at the peak velocity age, becomes markedly skewed as growth slows down, the slowing down being faster in some individuals than in others.

Boas also discovered from these curves that the earlier the spurt occurs, the more intense it is. Table 2, reproduced from his 1930 paper, shows this very clearly, as indeed do the figures from the 1932 paper below. The increment of stature for the year in which the peak velocity occurred averaged 10.0 cm in those with the peak at 12 to 13 years, descending regularly to average only 8.7 cm in those with the peak at 16 to 17 years. It seems that the general reactivity of the body declines as it grows older; but what en-

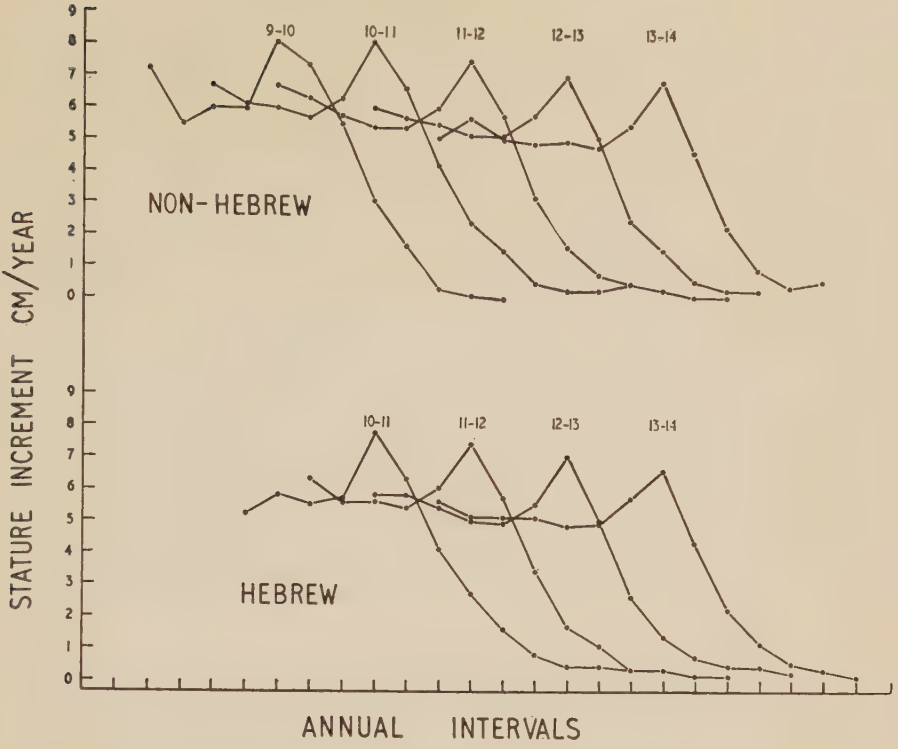


Fig. 5. Velocity curve for stature of Horace Mann School girls, Hebrew and non-Hebrew, classified by age at maximum rate of growth. The curves are spaced out diagrammatically along the time axis for clarity. (Boas 1932: Fig. 3 redrawn.)

docrinological or enzymatic or even cerebral mechanisms lie behind this generalization remains as yet unexplored.

Boas concludes this paper by posing a wider question that much occupied his thoughts, though not his experimental time. "In some individuals the whole physiological development . . . proceeds rapidly and energetically and the whole developmental period is short; in others it is sluggish and occupies

Table 2. Increments of Stature (Cm/Yr) of Newark Academy Boys During Intervals Around Year of Maximum Rate of Growth (Boas 1930)

| Maximum rate at years | −0.5 to +0.5 | −1.5 to +1.5 | −2.5 to +2.5 | −3.5 to +3.5 |
|---|---|---|---|---|
| 12–13 | 10.0 | 23.2 | 32.2 | 38.0 |
| 13–14 | 9.2 | 21.7 | 30.1 | 37.0 |
| 14–15 | 9.1 | 21.5 | 29.3 | 35.4 |
| 15–16 | 8.8 | 20.4 | 27.8 | 33.3 |
| 16–17 | 8.7 | 20.0 | 27.9 | 34.5 |

a much longer period. The question arises whether this is true for life as a whole . . . whether the life cycle of rapidly developing individuals is shorter than that of slowly developing ones." It is a question to which we have still no answer.

The 1932 paper documents these conclusions with more exactitude and detail. This time the data are mostly for girls, who are classified both in relation to age at maximum velocity as before, and age at menarche. Figure 4 (1932: Fig. 2 redrawn) is the equivalent for girls, classified by age at menarche, of Figure 3 for boys, classified by age at maximum velocity. It extends the conclusions further back into the growth period, for already at 7 years the early maturers are larger than the later maturers. Boas expressly regrets that he has no data for the earlier years, adding "we have to ask whether the earlier years belong in the same way to the more or less rapid developmental cycles which are found in the period from 6 to 17 in girls." Only recently has the first tentative answer to this been found, directly in the files of the Berkeley Growth Study (Nicolson and Hanley 1953) and indirectly in the Aberdeen longitudinal data (Tanner et al. 1956). It seems that between two and three years the characteristic growth pattern is established, both as regards tempo and size; before that events seem to be controlled to a major extent by factors not operating later.

Figure 5, also redrawn from the 1932 paper (Fig. 3), shows the velocity curves for groups of girls classified this time by age at maximum velocity. Boas divides his subjects into "Hebrew and non-Hebrew" to get an idea of genetic influences, but finds no difference. The data bring out well the way in which the intensity of the peak velocity declines in individuals whose peaks occur at the later ages. The other characteristic feature, that the interval between time of maximum velocity and of menarche is less for early, fast-travelling subjects than for later, slow-travelling ones, is brought out in a table.

The 1933 and 1935 additions to this paper contain little new information, except for a rather tentative investigation of the relation between dental maturity and advancement of growth in height. Using Hellman's observations, Boas shows that the time of eruption of the permanent second molars is to some extent related to the time of maximum growth, the regression being about four months advance in molar eruption for one year advance in peak velocity occurrence. For eruption of the permanent canines and premolars the relation was less marked, and the regression only one month for each year of maximum growth age. This result was a refinement of earlier observations made by Boas in Puerto Rico and reported by Spier (1918), demonstrating that at all ages from 7 to 13 boys who were advanced in the development of the permanent dentition were taller than those whose dentition was delayed.

Such is a bare summary of Boas' contributions to the study of human growth. There were, of course, various minor or incidental findings: that first-born children in Toronto in 1892 were larger at most ages than later-born, and that Californian (Oakland) children were even at that time larger at each age

than any others studied in North America (1895b); that height and weight of children at any age declines with increasing number of children in the family (1912b); that improvement in the regime of an orphanage in regard to food, exercise, and a healthy existence was reflected in the increased growth of the inmates (1935a); that despite the general recovery taking place after poor conditions have been rectified, eruption of deciduous teeth remains retarded, reflecting the events of months before (1927); that width of face increases with age from 20 to 50 years (1940b).

So much of Boas' work has a contemporary ring that it is easy to forget the general state of growth studies in 1930. Of the later famous longitudinal studies only the Harvard Growth Study (1922–34), the Iowa Station, and the Merrill-Palmer School had been long in existence, and little of consequence for physical growth had yet been published from them, except for the monograph of Baldwin. The Brush Foundation Study, the studies at Berkeley, the Fels Research Institute, the Yale Normative Study, the Center for Research in Child Development at Harvard, the Child Research Council at Denver—all were nonexistent or only very lately begun. All built on the solid foundation laid by Boas' technical knowledge, critical insight and personal wisdom.

## ENVIRONMENT AND HEREDITY: BIOMETRICAL GENETICS

Between these two "growth study" periods, Boas' interest was engaged principally in the question of the relative influence of environment and heredity upon body size and form. (The portion of Boas' interest allotted to physical anthropology, that is, was so engaged. So adequate to one man's entire life study were Boas' contributions to this section of anthropology alone that it is almost impossible to bear in mind that he was at least equally active in two or three other fields.) It is sometimes now forgotten that Boas sought equally methods for measuring environmental and hereditary influences. In *Race, Language and Culture* (1940c), the best-known selection of his papers, none of his contributions to biometrical genetics appears. Some other papers which are reprinted have even had the biometry cut out of them, presumably on the grounds that mathematics is an improper subject for students of anthropology. But in fact Boas made great and pioneer efforts, admittedly not always successful, to bring order into the dreadfully difficult matter of the inheritance of continuously distributed characters.

Already in his early field studies he was studying the distribution of anthropometric measurements in the parents and offspring of French-Indian crosses (1894b). To his surprise he found that the average stature of the half-blood offspring exceeded both parental averages. At this time he did not think of any environmental explanation, but remarked that "this curious phenomenon shows that size is not inherited in such a manner that the size of the descendant is intermediate between those of the parents, but . . . according to more intricate laws." The explanation of his findings is not now clear; a similar result is not shown by subsequent data on half-bloods. More importantly, Boas found that the distribution of face breadth, measured as bizygo-

matic diameter, seemed to be bimodal in the offspring. He fully realized that this appearance might be due to sampling error only, but said he believed it not to be, citing in evidence the very regular distributions he was accustomed to obtain in similar measurements of equal numbers of full-blooded Indians. He concluded that "the face of the offspring has a tendency to reproduce one of the ancestral types, not an intermediate type." This thought remained with him through much of his work on heredity, and perhaps influenced him to side with the Mendelians, mildly it is true, against the supporters of "blending" inheritance. We know too little about the inheritance of head and face measurements to pass a judgment on this finding even now, but we can say this: that of all the bodily measurements, those of the face are the best candidates for a mode of inheritance involving relatively few genes and some degree of dominance (Tanner 1953). Boas' pre-Mendelian hypothesis may yet turn out to be right, like his remarks of 1892 on the biometry of growth data.

In 1899(a), under the title of "*Some recent criticisms of physical anthropology*," Boas published an article of great interest in relation to his later thoughts concerning environmental influences on body form. He wishes to refute "a number of severe attacks against the methods of physical anthropology . . . which are directed mainly against two points—(i) the possibility of classifying mankind according to anatomical characteristics, and (ii) the practicability of descriptions of types by means of measurements." He vigorously defends the use of measurement. "Anthropological classification must be considered as a statistical study of local or social varieties." But he adds: "The tendency of developing a cast-iron system of measurements to be applied to all problems of physical anthropology is a movement in the wrong direction. Measurements must be selected in accordance with the problem we are trying to investigate. . . . Measurements should always have a biological significance. As soon as they lose this significance they lose their descriptive value."

He then goes on to say:

> The view that the skeleton cannot be used as a satisfactory basis for classification of mankind has been strengthened by the belief, frequently expressed, that the characteristic features of each race are not stable, but that they are influenced to a great extent by environment, geographical as well as social. It seems to me these views are not borne out by the observations that are available. . . . Each social unit consists of a series of individuals whose bodily forms depend on their ancestry and on their environment. If the opinion of the critics of physical anthropology regarding the predominance of the environment is correct, then we cannot hope to make any discoveries as to ancestry of local or social groups by means of anatomical investigations. If, on the other hand, it can be shown that heredity is the predominant factor, then the prospects of important discoveries bearing on the early history of mankind are very bright indeed. It seems to the writer that a biological consideration makes it very probable that the influence of heredity should prevail, and thus far he has failed to find conclusive proof to the contrary. . . . Criticism is directed against the permanence of types within the race; for instance against the permanence of short or tall statures, or against the permanence of forms of the head. . . . The actual proof [of the theory of predominance of the effect of heredity] must be looked for in comparisons between parent and off-

spring. . . . The method of this investigation has been developed by Francis Galton and Karl Pearson who have given us the means of measuring the degree of similarity between parent and child. Whenever this method has been applied, it has been shown that the effect of heredity is the strongest factor in determining the form of the descendant. It is true that thus far this method has not been applied to series of generations, and under conditions in which considerable change of environment has taken place, and we look forward to a definite solution of this problem of the effect of heredity and of environment through the application of this method.

Boas never at any time extolled the influence of environment as against heredity in the genesis of physical characters, as some people imagined. But the beginning of this passage was certainly his last defense of the classification of races by skeletal characteristics. It was not exactly that such a classification was erroneous, but simply that it became irrelevant as knowledge of genetics deepened. By 1931 Boas was writing, "Hereditary racial traits should be shared by the whole population so that it is set off against others." By this criterion whites and Negroes may be said to be different races, but among Europeans, and even between Europeans and East Asiatics, the overlap is too great for the word to be applicable. The idea of social groupings is now stated much more precisely: "The matter is quite different when individuals are studied as members of their own family lines . . . we may say that every racial group consists of a great many family lines which are distinct in bodily form . . . the differences between family lines belonging to each larger area are much greater than the differences between the populations as a whole" (1931).

The year 1899 saw a statistical study (1899b) on the cephalic index, seemingly more by way of a try-out of the correlational method on anthropometric data than the pursuit of any definite idea. Then in 1903, stimulated by the rediscovery of Mendel's Law in 1900, Boas wrote his first major contribution to biometrical genetics. This was on the heredity of the cephalic index, which he had measured in 48 Jewish families in New York.

At the time a controversy was in progress—not to use a more emphatic word—between those who since the publication of Galton's *Natural Inheritance* (1889) had been using biometrical methods in the investigation of human heredity, and those who accepted the new discovery of Mendelism and sought to apply its findings to human material. As so often, the controversy was based on an imperfect understanding of the natural phenomena involved, and its final resolution came through synthesis, not victory. The emphasis of the biometrists was on continuous variation in characters such as stature, and the early results of the Mendelians with their simple ratios and clear evidence of segregation of particulate units were thought to be irreconcilable with and hence inapplicable to these human results. "The understanding of continuous variation," writes Mather (1949), "awaited a fusion of the two methods of approach, the genetical and the biometrical, for each supplied what the other lacked. The one gave us the principles on which the analysis must be based; the other showed the way in which to handle continuous variation, the way of representing it in a form which made fruitful analysis possible."

Boas was much interested, of course, in Galton's and Pearson's work, but his findings of 1894 inclined him to accept Mendelism and the notion of "alternating" (i.e. taking after father or mother) rather than "blending" inheritance. He endeavored to develop equations for predicting the way in which the amount of the children's variation within different families depended on the degree of dissimilarity of the parents. One equation resulted on the assumption of "alternating," another on the assumption of "blending" inheritance. That his empirical results had sampling errors that made them incapable of distinguishing between the predictions of the two theoretical equations was nothing to be ashamed of: it is a result that has befallen generations of subsequent workers in this field with considerable regularity. Boas did apparently show that in his data the variation of the children increased with increasing dissimilarity between the parents, and he thought this evidence in favor of his old 1894 hypothesis: "that heredity of the cephalic index in individuals of the same race does not depend on the midparental value of the index, but that one-half of the children resemble . . . the father and the other half the mother."

Karl Pearson in the following year (1904) wrote a very friendly though rather damaging criticism. He used "Dr. Franz Boas' very suggestive paper" as a stimulus to pursue a good deal further the biometrical consequences of his own "law of ancestral heredity" and "law of reversion." He chides Boas for neglecting to consider the influence of grandparents and further ancestors in his equations, but acknowledges that "Boas' theory is more general than mine in that he assumes imperfect and not perfect correlation between the groups of offspring and the individual parents whom they respectively follow. This I consider a distinct gain." Pearson then gives more general equations for the dependence of the variability of offspring upon parental dissimilarity for three theories: (i) Law of Ancestral Heredity, which gives equal variability of offspring whatever the parental difference and leads to a regression of offspring on parent which is both linear and homoscedastic; (ii) Mendelian theory, which Pearson says leads to an increase of variability with increasing parental difference, "unless the number of Mendelian couplets on which the character depends is indefinitely great" (here, then, the ultimate resolution of the controversy lies implicit); (iii) "Individual parental dominance" of Boas (1894b), according to which the variation in offspring is related to parental difference in a hyperbolic fashion. On his own data for stature, cubit, and span (Pearson and Lee 1903), he finds that the first hypothesis is borne out. Amusingly enough, he finds also that his data would fit the second, Mendelian, hypothesis if "forty-eight couplets" or more were involved. For the cephalic index he has measurements of 2,000 pairs of brothers; the correlation is 0.49, and a test similar to the parent-offspring one can be made, which leads once more to acceptance of the first hypothesis and rejection of the other two. In discussing the discrepancy between Boas' results and his own, Pearson remarks that Boas calculated the within-family offspring variability wrongly, using the general instead of the individual family means, and also that some of his

standard deviations were calculated on the basis of only two or three subjects which would give an erroneous result. (The number of subjects $N$ was still used in calculating the standard deviation at this time, and not the degrees of freedom, $N-1$.) Inspection of Boas' figures inclines one to believe that these criticisms were indeed well founded.

By 1907 Boas had extended his material to 192 families. Following his usual custom when any controversy arose, he published all the raw data, though rather surprisingly he does not quote Karl Pearson's paper. (Boas seldom quoted literature in his papers, except when directly using a result or criticizing a paper: he does make reference to Pearson's paper in a later article [1912b] in which he replies to a whole series of criticisms of his work on immigrants.)

In Table 3 the familial correlations found in this and some later studies are given. The 1912(b) subjects presumably incorporate the 1907 ones, and are for the most part immigrants and their children. Children of five years and over were used, their values being corrected to estimated adult ones by means of the average figures from the Worcester growth curve. Boas recognized that this entailed considerable error, particularly for the earlier ages and at adolescence. The striking thing about these correlations is that they are lower than we would now expect on the basis of a many-gene no-dominance mechanism, which leads to parent/offspring and sib/sib correlations of 0.5. Part of the lowness may come from the inefficiency of the age correction, and part from the differential influence of environment on parents and offspring, especially in the case of the immigrant families. The really very curiously low stature correlations presumably must be due to these causes. Boas later had two of his students repeat some of the observations and in 1928(a) published the series of correlations displayed in the lower panel of Table 3. His later results, in opposition to his earlier ones, and in confirmation of Pearson's figures, gave no relation between fraternal variance and parental dissimilarity. But the finding of a sib/sib correlation for cephalic index consistently higher than the parent/offspring in all the series, early or late, constitutes a strong claim for the occurrence of dominance or else interaction between nonallelic genes.

It is interesting to see how Boas summed up the situation in human biometrical genetics shortly before the publication of R. A. Fisher's great paper of 1918, which more than any other reconciled biometry and Mendelism and laid the basis of modern biometrical genetics. In discussing how far a new physical type might result from the mixture of populations going to make up the American population, Boas (1917) wrote:

> Galton and his adherents maintain that in a mixture of types a new intermediate type will develop analogous to the appearance of the mule as a result of mixture between horse and donkey. Other investigators following the important observations of Mendel and his successors, claim that no permanent new type develops, but that the so-called "unit" characters of the parents will be segregated in the mixed population. Assuming, for instance, the blue eye of the North European to be a "unit" character, it is presumed that in the mixed type there will always remain a certain group with blue eyes. . . . In these cases there must be a certain degree of alternating inheritance, in so far as in

TABLE 3. FAMILIAL CORRELATIONS FOR HEAD MEASUREMENTS AND STATURE REPORTED BY BOAS ON VARIOUS GROUPS OF SUBJECTS

| Relationship | Head breadth | | | Head length | | Cephalic index | | Stature |
|---|---|---|---|---|---|---|---|---|
| | 1907 | 1912(b) | 1913 | 1907 | 1912 | 1907 | 1933(b) | 1913 |
| Father-son | 0.21 | 0.38 | | 0.24 | 0.46 | 0.20 | 0.26 | |
| Father-daughter | 0.23 | 0.36 | | 0.34 | 0.40 | | | |
| Mother-son | 0.33 | 0.35 | | 0.20 | 0.28 | 0.25 | 0.34 | |
| Mother-daughter | 0.27 | 0.35 | | 0.46 | 0.41 | | | |
| Sib-sib | | | 0.56* | | | 0.42 | | 0.33 |
| Parent-offspring | | 0.34 | | | 0.38 | | | 0.21 |

1907 192 Jewish families; values of children (over 5) adjusted by average growth curve. Father-son, -daughter, mother-son, -daughter, each approx. 250 pairs; cephalic index 630 pairs; sibs 614 pairs.

1912(b) (p. 159): Jewish families, including immigrants. Children's values adjusted as in 1907. Pooled values for foreign-born and American-born children. Father-son, -daughter, approx. 500 pairs each, mother-son, -daughter, approx. 650 pairs each. Parent-offspring 2,327 pairs.

Note that some possibly significant differences exist between parent/foreign-born offspring and parent/American-born offspring correlations in case of father/daughter length of head ($.24 \pm .06$ and $.55 \pm .04$ resp.) and mother-son length ($.18 \pm .06$ and $.36 \pm .05$) and breadth ($.43 \pm .05$ and $.28 \pm .05$) of head. No division given for sibs or for stature.

1913: Same data as 1912.

1933(b): Frets Dutch data: 283 families with 1348 children. Children's values adjusted by use of growth curve of material itself.

* For "head measurements."

TABLE 3 (LOWER PANEL). FROM BOAS 1928(a)

| | Cephalic Index | | | | | | | Bizygomatic Diam. | |
|---|---|---|---|---|---|---|---|---|---|
| | Bohemians | Poles | Hebrews | Sicilians 1910 | Sicilians 1926 | Negroes | Italians | Bohem. | Sicil. |
| Correlation, sib-sib | 0.36 | 0.44 | 0.29 | 0.41 | 0.32 | 0.31 | 0.50 | 0.61 | 0.47 |
| Correlation parent-offspring | 0.16 | 0.24 | 0.24 | 0.23 | 0.20 | 0.23 | 0.29 | 0.33 | 0.32 |
| Variance | 10.90 | 11.81 | 9.04 | 10.36 | 10.90 | 10.75 | 12.47 | 33.8 | 25.5 |

a mixed population some individuals will resemble in their traits the one parental race, while others will resemble the other. Some investigators claim that the existence of this type of inheritance—so-called "Mendelian" inheritance—has been definitely proved to exist in man. It is hardly possible at the present time to answer this important problem with any degree of definiteness, although in regard to a number of traits [such as eye color] sufficient evidence is available. Whether or not the classical ratios of Mendelian inheritance prevail is a question that it is quite impossible to answer. On the whole it seems much more likely that we have varying types of alternating inheritance rather than true Mendelian forms [i.e. forms giving simple 3 to 1 ratios etc.].

Thus Boas sided with the Mendelians so far as the fundamental of segregation was concerned, but found their ratios simply inapplicable in the field he was most interested in. He returned to a slightly different sector of the field with his papers of 1916 and 1928, discussed below; but in the meantime he turned his attention toward another approach to the problems of heredity and environment.

*Children of American Immigrants*

The study of immigrants to America and their children, vast in scope, unexpected in outcome, and staggering in professional and public reception, began in 1908. It was made for the United States Immigration Commission, whose object seems to have been to determine whether the influx of immigrants was resulting in a deterioration of the physique of the American population. Boas with some thirteen assistants, prominent among whom was Crampton, measured the stature, head length and breadth, bizygomatic diameter, and in some cases the weight, of a total of nearly 18,000 persons in New York. All were immigrants or the American-born children of immigrants. Of these, some 5,500 were aged 25 and upward, and the remainder children over the age of four. The largest number were East European Jews (about 6,000) and Bohemians, Sicilians, and Neapolitans (about 3,000 of each), with smaller groups of Poles, Hungarians, and Scots. The material was almost exclusively family groups, the main comparison being between children born before the parents immigrated ("foreign-born") and hence growing up in Europe, and children born in America after the parents had come to New York.

The main report was issued in book form in 1912(b) under the title *Changes in Bodily Form of Descendants of Immigrants.* In the same year (1912c) a summary of the main findings was published in the American Anthropologist, together with answers to some of the criticisms already encountered. The criticisms were to continue, off and on, for years, and in 1928(b) the entire raw data were issued, constituting the largest collection of family measurements ever published.

Throughout the study the main emphasis was laid on the cephalic index. Boas himself seems to have been a little astonished at the results ("the investigation has shown much more than was anticipated") but this was nothing compared to the astonishment of other anthropologists. "The head form," he wrote, "which has always been considered one of the most stable and perma-

Table 4. Mean Differences in Anthropometric Dimensions between Foreign-Born and American-Born Children

| Nationality and sex | Length of head (mm) | Width of head (mm) | Cephalic Index | Width of face (mm) | Stature (cm) | Number |
|---|---|---|---|---|---|---|
| Bohemians: | | | | | | |
| Males | −0.7 | −2.3 | −1.0 | −2.1 | +2.9 | 170 |
| Females | −0.6 | −1.5 | −0.6 | −1.7 | +2.2 | 180 |
| Hebrews: | | | | | | |
| Males | +2.2 | −1.8 | −2.0 | −1.1 | +1.7 | 654 |
| Females | +1.9 | −2.0 | −2.0 | −1.3 | +1.5 | 259 |
| Sicilians: | | | | | | |
| Males | −2.4 | +0.7 | +1.3 | −1.2 | −0.1 | 188 |
| Females | −3.0 | +0.8 | +1.8 | −2.0 | −0.5 | 144 |
| Neapolitans: | | | | | | |
| Males | −0.9 | +0.9 | +0.9 | −1.2 | +0.6 | 248 |
| Females | −1.7 | +1.0 | +1.4 | −0.6 | −1.8 | 126 |

Differences calculated within each yearly age group and weighted by number in each group.

nent characteristics of human races, undergoes far-reaching changes coincident with the transfer of the people from European to American soil." He significantly and bafflingly added, "The East European Hebrew, who has a very round head, becomes more long-headed; the South Italian, who in Italy has an exceedingly long head, becomes more short-headed; so that in this country both approach a uniform type. . . . These results are so definite that while heretofore we had the right to assume that human types are stable, all the evidence now is in favour of a great plasticity of human types, and the permanence of types in new surroundings appears rather as the exception than as the rule." This quotation somewhat overstates his case and could in fact be toned down by other quotations from his book. But it upset the anthropologists. When rather incautiously he continued, still on page one, by saying: "We are compelled to conclude that when these features of the body change, the bodily and mental make-up of the immigrants may change," this upset some others of various persuasions.

The main results are reproduced in Table 4, where the differences in response of the four groups are well brought out. Only in the decrease in bizygomatic diameter do all the groups agree. Surprisingly, the Italians even fail to increase in stature, which is perhaps a warning that conditions in New York may not have been altogether better than in Italy, as Boas seems throughout to have assumed. Sunshine and a hot climate are at least two unconsidered factors. Though only the means over all ages are given in the table Boas'

graphs show quite clearly that the effects are present by five years and persist at about the same intensity up to adulthood.

Boas backs up this data with three further analyses, showing (1) that differences similar to these in direction and size occur between parents and their own American-born children, and that these differences are greater than differences between parents and their own foreign-born children; (2) that differences between parents and their American-born children are greater for children born after the parents have been ten years in America than for children born soon after the parents' arrival; and (3) that similar differences are shown between individuals born in Europe in year X and children of people who left Europe in the same year X.

This third supplementary finding was designed to answer the criticism that secular changes in the European population might account for the results. As a matter of fact, Boas' graphs incline one to believe that there was a considerable secular change going on among the European sources of immigrants during the period in question, at least in face width though perhaps not in head length and breadth.

As to the causes of these phenomena, Boas confesses himself baffled, beyond the vague supposition that they are environmental in origin. "My position is that I find myself unable to give an explanation of the phenomena, and that all I try to do is to prove that certain explanations are impossible. . . . I am more inclined to ask for further material from other sources than to force a solution that must be speculative" (1912b). The only information about the mechanism by which the changes were brought about was provided by Crampton's contribution: very interestingly, it was found that the differences in stature between American- and foreign-born still persisted even when the two groups were equated for stage of pubescence rather than for chronological age. Thus the differences came not from an alteration in tempo, but an alteration in actual size for given developmental age.

Various, mostly critical, explanations were advanced by other authors. They ranged from poor measurement technique to a high illegitimacy rate. This latter explanation, put forward by a writer in the Edinburgh Review of 1912 and repeated with supporting calculations by Fisher and Gray in 1937, would indeed provide one of the few conceivable explanations for the curious movement toward indigenous American figures from both extremes of foreign cephalic index. The resemblances of American-born immigrants, of whatever stock, to local Americans would receive a rational, if unsuspected, explanation if some of the local Americans were the true fathers. Boas foresaw this criticism and endeavored to rebut it in advance by showing that the correlation coefficient for cephalic index between fathers and American-born sons is 0.39 and fathers and foreign-born sons 0.37. While all right so far as it goes, this measure is not really sensitive enough to settle the question. Nor, for that matter, are Fisher and Gray's calculations, which show a somewhat higher regression of children's measurements on mothers' than on presumed fathers' values, but with a difference not actually reaching the level of statistical significance.

The chief fault with this vast array of data—and there are some distinct curiosities in it—clearly lies in the lack of any systematic sampling technique. The importance of this was not realized at the time the study was made and no discussion is devoted to it beyond the observation that stratification by national groups was thought advisable so as to achieve substantial numbers of homogeneous populations.

However these criticisms may be, nowadays we would view Boas' results and conclusions with equanimity, if a little puzzlement. The changes he described were by no means great: only a half to one inch in stature, one to two mm—that is about 1 percent of the mean or about 6 percent of the range—in head length and breadth. They are of just about the magnitude that later studies of nutrition, war famine, social class differences, and migrant/nonmigrant divergence would lead us to anticipate in a group undergoing marked changes in environmental circumstances during growth (see e.g., Brues 1946, Lasker 1946, 1953, Tanner 1955). Boas himself fully realized the smallness of the changes observed, despite the quotation given above. He specifically says (1912b) that "it would be too much to claim that all the distinct European types become the same in America, without mixture, solely by the action of the new environment." The plasticity of the type is "strictly limited." He added later (1916b) that there was "nothing to indicate these changes are in any sense genetic changes; that is to say that they influence the hereditary constitution . . . it may very well be that the same people if carried back to the old environment would revert to their former physiological types." Later still, in discussing the work as a whole (1936), he said:

> It has been known for a long time that the bulk of the body as expressed by stature and weight is easily modified by more or less favourable conditions of life. In Europe there has been a gradual increase in bulk of body between 1850 and 1914. Adult immigrants who came to America from south and east Europe have not taken part in this general increase . . . presumably because they were always selected from a body the social condition of which has not materially changed. Their children however born in America or who came here young have participated in the general increase of stature of our native population. . . . With this go hand in hand appreciable differences in body form. . . . These changes do not obliterate the differences between genetic types but they show that the type as we see it contains elements that are not genetic but an expression of the influence of environment.

With such a statement we could hardly quarrel nowadays: only the size of those certain elements under various circumstances remains in dispute. But while the results on stature passed into the literature with scarcely a murmur of dissent, alterations in cephalic index were quite another matter and anthropologists fought against them then as we should fight against alterations in blood groups now.

It is hard for us to realize the degree to which the anthropologists of the last century were obsessed with the constancy of the cephalic index. Head form appeared to them a method of tracing the origins and distributions of peoples; if it altered in response to environment, a major prop was kicked out from

under the anthropological pile-dwelling. There was no knowledge of blood groups in those days and the wish, as usual, tended to be father to the thought. Boas himself expressed this feeling, it will be remembered, in his 1899 paper, which in retrospect sounds like a temporary reaction against papers published in 1892 and 1896 by the great Virchow (see Ackerknecht 1953:213). Boas had been an assistant at the Berlin Ethnological Museum from 1883 to 1886 and came strongly under the influence of Virchow, who was even then one of the few anthropologists who upheld the idea of "the plasticity of races and the possibility of change in cranial indices" (Ackerknecht 1953). Thus Boas must have been quite prepared for his findings on the immigrants' children and was not unduly shocked as were the majority of his contemporaries. He himself never lost sight of the relative smallness of the environmental effect he believed he had demonstrated and never in fact subscribed to the absurdly environmentalist views seemingly attributed to him equally by those in favor of and those against them. It is significant that when, with typical integrity and impersonal good sense, he published all the raw measurements themselves for others to work up and interpret as they pleased, he entitled the publication *Contributions to the Study of Inheritance.*

## FAMILY LINES

Boas was never one to attack a problem from only one point of view or to rest content with a one-sided solution. By 1916 he was back studying the more genetical side of the genesis of physique. He had been struck by Johannsen's experiments on pure lines of beans described in 1909 in the *Elemente der exakten Erblichkeitslehre.* Johannsen had shown that heritable and nonheritable agencies were jointly responsible for the nearly continuous variation in seed weight which he observed and that only a breeding test could distinguish them. The action of environment in smoothing out discontinuity in the genotype to produce continuous variation in the phenotype was thus becoming recognized.

Boas at once saw the relevance of this work to human genetics and in 1913 he made a somewhat abortive attempt to estimate the relative contributions of hereditary and nonhereditary factors to variation in anthropometric measurements. To do this he had to assume that the "hereditary part of the similarity [between relatives] is the same for all parts of the body, an assumption that needs proof. . . ." He then argued that the head measurements were less exposed to environmental influence than stature, since the head had less of its growth to complete after birth. The difference between parent/offspring or sib/sib correlations for head measurements and stature could then lead to an estimate of the hereditary contribution. He realized far too many and too simple assumptions were involved and soon dropped this approach: nevertheless, it was a courageous attack on a central fortress, even if made with sticks and stones. What he does not mention, however, is that head dimensions might reflect the environmental effects on the foetus through the mother. Neither does the idea of using twins to solve his problems seem to have occurred to him.

Boas' 1916(a) paper is concerned with the estimation of the degree of inbreeding in a population, a topic of great importance, then as now, in elucidating the classical anthropological problems of the movements, distributions and descents of peoples. Johannsen made clear, Boas says, that "the correlation between parents and children will be the lower, the more uniform the lines composing the population. . . . Strong family resemblances in a population are indications of heterogeneity of the composite lines of descent." Boas makes much use here and later of the phrases "family lines" and "lines of descent," which he equates with Johannsen's pure, that is inbred, lines. He splits the total population variance into what we would now call within-fraternities and between-fraternities. The between-fraternity variance measures the heterogeneity of lines of descent and is called the "family line" variance. In an inbred line it will be vanishingly small compared with the within-fraternity variation, provided environmental circumstances are similar for all families.

Boas shows that in families of $m$ siblings the family-line variance is given by

$$s_l^2 = \frac{1 + (m - 1)r_{ss}}{m} \sigma^2$$

where $r_{ss}$ is the sib/sib correlation, and $\sigma^2$ the total population variance. For a fraternity with a very large number of children, therefore, $s_l^2 = r_{ss}\sigma^2$. Also, on this assumption

$$r_{ss} = \frac{s_l^2}{\sigma^2}$$

and thus measures the degree of inbreeding (or more accurately, of "outbreeding").

Lacking the technique of analysis of variance, which was to come into use

Table 5. Partition of Variance of Cephalic Index into "Fraternal" and "Family-Line" Portions

| | Between-fraternity: "family-line" | Within-fraternity: "fraternal" |
|---|---|---|
| Bohemians | 2.37 | 2.61 |
| Central Italians | 2.39 | 2.72 |
| East European Jews | 2.29 | 2.52 |
| Scotch | 2.17 | 2.66 |
| Dutch | 1.95 | 2.33 |
| South African Bastards | 1.26 | 2.52 |
| Missisauga Indians | 1.47 | 3.10 |
| Other Chippewa Indians | 1.77 | 3.32 |

Figures are standard deviations in mm. From Boas (1913, 1933b).

only in the early 1920's, Boas had some trouble calculating the between-fraternity variance for families of different size, but he managed to arrive at the table of family-line and within-fraternity standard deviations for cephalic index, as indicated by Table 5.

The South African Bastards, the figures for whom Boas obtained from Eugen Fischer's unpublished tables, are known to be greatly inbred, and the Missisauga and other Chippewa Indians inbred to a lesser extent. Thus results in general agree with theory, and Boas concluded that "it appears from these few samples that the variation in lines of descent [i.e. degree of inbreeding] in Europe seems to be very uniform." In this paper, however, he ignored the effects of environment almost totally, or rather he implicitly assumed that they were the same between as within fraternities. He made, in fact, the common assumption of lack of interaction between genetic and environmental influences which subsequent generations of biometrists have also found so extremely difficult to do without. Consequently his measure of inbreeding estimates the degree of uniformity of environment between families as well as the degree of uniformity of genes.

This pioneer work on the restriction of the fraternal correlations by inbreeding was largely ignored, in 1916 and later. It was made, one must remember, two years before Fisher's fundamental paper, and nearly twenty years before Hogben (1933) and others began the systematic study of the fraternal and parent-offspring correlation in human characters under various conditions of mating and of mode of inheritance. In 1928(a), in his last theoretical paper on biometrical genetics, written when he was seventy, Boas returned to the estimation of the relative contribution of heredity and environment, though in a roundabout and rather questionable way. This time he gives the effect of inbreeding on the parent-offspring correlation, again citing Johannsen, and pointing out that if $\sigma_h^2$ is the between-line variance for pure lines, then in a group of subjects taken from a mixture of such lines

$$r_{po} = \frac{\sigma_h^2}{2\sigma^2}.$$

If the family lines as determined from the sib/sib correlation are the same as these "hereditary family lines" the fraternal correlation would be twice the parent-offspring. In fact this is not so in his figures, the fraternal being about one and a half times the parental. The difference, he says, must be due to the "differences of new family lines established by the members of a fraternity," and if the variance of these new lines is $\sigma_L^2$,

$$\begin{aligned}\sigma_L^2 &= \sigma_l^2 - \sigma_h^2 \\ &= \sigma^2(2r_{po} - r_{ss})\end{aligned}$$

The total variance is then split into $\sigma_l^2$, the family-line, or between-fraternity variance, $\sigma_L^2$ the new-family-line variance, and the "non-hereditary" variance, the remainder.

The paper is not easy to follow and exactly what Boas had in mind, in relation to modern terminology, is not very clear to the writer. The results are certainly questionable. Boas himself ended by writing, "The method contains many simplifying assumptions that do not correspond to reality. It seems however possible that the ultimate problem may be attacked by a further elaboration of this method." He seems to have been unaware that such elaboration had actually been in progress for a decade, and in this ignorance he was in company with the vast majority of human and animal biologists of the time.

Boas' position in biometrical genetics was perhaps less masterly than in other fields, though not less pioneering. To begin with, as the subject was unfolding, he was up with the leaders. But as the pace grew hotter his relative lack of analytical skill in mathematics and training in Mendelian genetics pulled him back behind those who were setting the pace.

Perhaps this is just to say that Boas belonged to the generation of Pearson, not of Fisher and Hogben and Haldane and Sewall Wright. Nevertheless he was one of the very few persons who brought real technical skill and knowledge to the investigation of both sides of the culture versus genetics problem. Perhaps that is why his attitude to this question seems in retrospect so balanced. His understanding of genetics and his investigation of family lines gave him an insight into the true biological structure of populations denied to his race-ridden, cephalic-index-loving contemporaries. In the bridge between the older concepts of physical anthropology and the newer disciplines of human biology, Boas' work constitutes much of the framework.

In the atmosphere of his time Boas found it necessary to spend some of his energy refuting current notions about "racial differences." To this task, best carried out in *The Mind of Primitive Man* (1938), he brought knowledge and great common sense. While regarding racial classifications as almost totally irrelevant, and considering culture much more than heredity the chief determinant of most psychological traits, he yet wrote (1931):

> There is no doubt in my mind that there is a very definite association between the biological make-up of the individual and the physiological and psychological functioning of his body. The claim that only social and other environmental conditions determine the reaction of the individual disregards the most elementary observations. . . . There are organic reasons why individuals differ in their mental behaviour . . . but to acknowledge this fact does not mean that all differences of behaviour can be adequately explained on a purely anatomical basis.

## TEMPO OF GROWTH OF FRATERNITIES

In what was almost his last paper (1935b) on these matters, Boas blended together his interests on growth, physical form and genetics. The paper began, "It seems most important that by organized effort the life history of individuals should be followed up in order to show whether the rate at which physiological development and decay occur are constitutionally determined."

Boas then showed from his Hebrew Orphanage and Horace Mann School material that there was a resemblance in tempo of development between sib-

lings. Taking stature at 7, 9, 11 and 13 as a necessarily rough indication of advancement or retardation, he classified the children into three groups. He then plotted the velocity curves of the siblings of children in each group, and observed the siblings of the early-maturing themselves had an early-occurring maximum velocity, the siblings of the late-maturing a late-occurring one. He went on to cite the secular trend and his own observations on the effect of change of regime on growth at the Orphanage as evidence that the environment also affects growth. While by no means abandoning his earlier position that tempo of growth reflected social and environmental conditions, he now, as so often, restores the balance by ending, "All these observations may be summarized in the statement that each individual has by heredity a certain tempo of development that may be modified by outer conditions. The gross, generalized observations available at the present time suggest that in a socially uniform group the tempo of development may be considered as a hereditary characteristic of individuals" (1935c).

## SUMMARY

Such, then, is a summary of the widespread and manifold influence which Boas exerted in the field of human growth and form. His demonstration of the statistical distortions to which cross-sectional growth data were necessarily subject made inevitable the longitudinal studies which have been the chief characteristic of progress in the field during the first half of this century. His discovery of the individual differences in tempo of growth, and his later demonstration of how they might be handled statistically, had repercussions in child psychology, sociology, and education. Much of the effect of this work has yet to be realized, but gradually the notion of individual advancement or retardation in tempo, and the idea of physiological and psychological variability among children of the same chronological age, are making themselves felt in the schools and in the home: this also we owe fundamentally to Franz Boas.

His work on the children of immigrants to America gave the death-blow to belief in the fixity of head form, and was a considerable factor in bringing anthropologists to the modern biological viewpoint which sees heredity and environment as complementary in shaping physique. His efforts at unravelling the mechanisms of inheritance of continuously distributed human characters gave him an insight into population biology which was altogether modern in flavor, and which enabled him quite against the current of his time to assign to limbo the question of racial differences and replace it with statistical queries on the number and distribution of family lines.

Some of Boas' findings, of course, have not been verified by later events. Some of his interpretations need amendment. Sometimes he too failed to see all that lay implicit in his data. The astonishing thing is how seldom a questionable finding, a dubious interpretation, or a missed opportunity found its way into Boas' work. He was a remarkable innovator, who nevertheless made few mistakes—and no major ones. He was a man of enormous width of interest, yet his writings in this small sector show no trace of a dispersion of talent. On

the contrary, most of his work makes the other writers of his time look like dilettantes; he it is who penetrates below the surface.

Much of his influence was due to this critical quality and to his biometrical training. Much, also, was exerted outside his published papers, numerous though they are. When Crampton's 1908 paper on physiological age was reprinted in 1944, Crampton wrote a short postscript. It said, "The author [i.e. Crampton] is commonly given credit for originating the term 'physiological age.' This, I believe, properly belongs to the much respected and beloved Franz Boas." Many others might have written the same of their contributions to human biology in the first half of this century.

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# *Boas as Statistician*

W. W. HOWELLS

IT IS not easy to add anything to Herskovits' accounts (1943, 1953) of Boas' contributions to physical anthropology or to the use of statistics. This is particularly so since, as Herskovits accurately says, Boas' statistics did not stand as a system by themselves (as was the case with Pearson), "the development of the requisite mathematical formulae being almost in the nature of by-product of his preoccupation with the problems of human physical form" (Herskovits 1943). That is to say, Boas did not employ statistics primarily as a tool or form of approach to material, but rather as a means of projection, as a second language, in stating his ideas or demonstrating his results. It is clear that Boas was well trained in mathematics and much at home in the subject; and it is clear that this second language was Greek to most of his anthropological contemporaries, at least in this country. Using mathematics as a form of logic, it seems likely that Boas was led into lines of thought and kinds of enquiry which his unmathematical colleagues neglected. We might ask briefly how, in general, he applied statistics for his purposes, and why he did not influence his contemporaries.

Boas the somatologist is remembered by anthropologists in general entirely for his *Changes in Bodily Form of Descendants of Immigrants* (1911). In this, he showed that measured physical characteristics need not be immutable when a human population is transplanted from one environment to another, and for illustration he used that sacred cow, the cephalic index. This amounted to loosening a key foundation stone in the structure of assumptions used by students of racial history; for anthropologists were convinced that head form was a basic physical attribute that would manifest itself even in racial mixture, either by segregation or by contributing to the hybrid in such a way as to indicate the parental stocks with some reliability. Such an assumption, in this exaggerated form, was specifically asserted by Sergi and Dixon, and was certainly subscribed to widely by others.

Boas' findings were therefore something everyone could understand and respond to, the more so as he presented his material simply. He merely made repeated comparisons, at different ages and of different groups of native and foreign-born subjects, showing by its consistent appearance the reality of the change in immigrants. In a short chapter on the element of heredity, Boas gave correlations between parents and children, but in the discussion of the main theme, beyond his graphs and his plus and minus signs, the demonstration needed no statistical elaboration and got none.

But Boas was also interested in more complex problems which called for more complex mathematical statement. Note his development of a purely biological idea, the partitioning of the variation in a measurement. Writing "On

the variety of lines of descent represented in a population" (1916), he said that "in a population in which the lines of descent are the same for each fraternity, we should expect the averages of these fraternities to be identical, provided the fraternities are large enough. In populations in which the lines of descent are quite distinct, we should expect the averages of the fraternities to differ strongly among themselves. *In other words, the variability of the averages of the fraternities will be a measure of the heterogeneity of the lines of descent.*" (Italics mine.) He stated this perhaps even more explicitly later (Boas 1931).

He had used the same idea in 1913, in pointing out the need for distinguishing sharply between hereditary and environmental influences, while he was making an estimate of these influences from the correlation of parents and children (1913). He stated this in the same general statistical form in the abovementioned papers, including one of 1933 (Boas 1933). In the last, he was obtaining the variances of fraternities and families directly, and was approximating the statement that the variance of the population was the sum of these variances.

In other words, he was applying the analysis of variance, so much used today. Although Fisher did not introduce its application to agricultural field experimentation until 1923 in a paper with MacKenzie (Fisher and MacKenzie 1923), Boas can hardly be said to have invented it. The notion of partitioning the variance was already known to others. In 1889, Galton, in *Natural Inheritance*, had discussed the relationship between the variation of brothers about their family means to the variation of the family means about the population mean, and Boas cannot have been ignorant of this. Also, Boas did not carry forward analysis of variance as a general statistical tool, as Fisher and Snedecor did. (In fact, Boas himself seems to have been so little impressed with anything but the anthropological ideas involved that, in the 1940 edition of reprints, the 1913 article appears in a curiously inaccurate translation, with the mathematical treatment left out entirely, while none of the other articles cited above appears at all.) The point is that Boas, with his clear understanding of the problems of variation and his statistical mode of thinking, was long ago stressing certain important points, obvious enough now when analysis of variance is the order of the day, and had stated them in a mathematically appropriate fashion, but according to his own formulations.

Thus a good deal of the statistics seems to have been original with Boas, although he was not working in isolation. He was evidently in tune with the best thinking of the late 19th century on the nature of variation, and such thinking naturally took biometric forms. Boas was an admirer and follower of Galton, whom he had met (Lowie 1943, 1947), and this early stimulus must have added considerably to his natural bent in the development of biometrics. In 1896, Galton's disciple, Pearson, published numerical methods of correlation and Boas adopted them. In fact, Boas used $r$, the Pearsonian coefficient of correlation, in his formulae for partitioning the variance. In 1909 he published a purely statistical article (Boas 1909), which suggested three other methods of calculating a coefficient, including one for nonmeasured data. Pearson (1909)

at once replied, pointing out that these were variants he himself had already presented in slightly different form, and which are not true correlations of the product-moment variety. To paraphrase Pearson, one of them is related to his "coefficient of contingency" and to chi-square, and another amounts to what is now called the "intra-class correlation," which in turn corresponds to an estimate of the proportion of the between-group variance, as for example the between-family variation in Boas' studies. Boas ignored Pearson's comment, continued to use the method, and developed the same idea again in 1921 (Boas 1921).

A second example of Boas' approach illustrates both his anticipation of later ideas and his intellectual relationship to Galton and Pearson. In 1894, writing on the subject of defining types by measurement, he said:

> The measurements ought to be selected and treated in such a manner that those which are independent of each other may be recognized as such, and that typical correlations be brought out clearly.
>
> Any anatomical or physiological measurement of an organism may be considered a function of the general conditions of heredity and environment affecting the measured individual as a whole and in those parts which have been subjected to measurement. If two such measurements be called M and $M_1$, the conditions are $a_1, a_2, a_3 \cdots a_n$; we have
>
> $$M = f(a_1, a_2, a_3 \cdots a_q);$$
> $$M_1 = f_1(a_1, a_2, a_3 \cdots a_q)$$
>
> The variable conditions $a_1, a_2 \cdots a_q$ may be divided into three classes: those which influence M alone . . . ; those which influence $M_1$ alone, . . . ; and those which influence both M and $M_1$ . . . (Boas 1894).

Boas went on to illustrate the phenomena of regression and correlation, from simple numerical examples, rather than by generalized algebraic statement. In 1888, Galton had already arrived at a similar idea: that two measurements might be correlated through the effect of "common causes" (Galton 1888), and had proposed a formula for correlation. He envisaged multiple correlation as well, though he did not know how to develop it (Burt 1949); and in 1889 he dwelt at length on the matter of regression. Thus both men were stating the germ of what is now generally approached through factor analysis, i.e., detection and specification of the background factors which cause correlation among several measurements.

Then Pearson (1896) introduced the product-moment coefficient of correlation and restated the notion of independent causes contributory to the variation of a measurement, in terms similar to those quoted from Boas, who borrowed Pearson's new developments, extending the use of multiple correlation to four characters (Boas 1899). This article dealt with the cephalic index. Boas pointed out that this was a complex trait, and also that length, breadth, and height of the skull all varied largely independently of each other, and contributed separately to cranial capacity. He showed how each of these measurements could be viewed as the result of varying causes and random errors, and also how each could be stated in terms of these and of its correlation with the

others; he noted especially that some measurements seemed more closely associated with capacity and others more independent. All this was done by use of up to quadruple correlations among the measurements, and by showing the extent to which variation of each measurement was diminished by removing the influence of its correlation with the variation of the others.

This is actually primitive factor analysis. Here we have Galton suggesting the ideas of unobservable causes or factors and of correlation; Pearson taking up the ideas and developing correlation coefficients; and Boas applying small-scale multivariate analysis to head measurements, through multiple correlation.

Why was this not followed up? Boas returned several times to the cephalic index and to the question of its heredity, but he did not use the same treatment. Again, Boas seems to have developed an ad hoc method of analysis or expression but did not use it systematically or repeatedly, and so did not establish it as a statistical method in the manner of Pearson or Fisher. (Apparently his 1909 venture into methods of correlation was not a success.) As to factor analysis itself, in 1901 Pearson proposed the actual extraction of principal components, but stopped there. Accordingly, factor analysis did not penetrate further into anthropology, and was eventually taken up instead by Spearman and the psychologists.

There were other hazards to Boas' usages. The ideas he was interested in, however congenial they may have been to plant geneticists of the time, were out of joint with the interests of physical anthropology. They sound thoroughly up-to-date now, when genetic analysis of quantitative traits, necessarily by mathematical methods, is recognized as a major problem. It is another sign of his astonishing intuition, awareness of problems, and independence of thought that, during the period of the rediscovery of Mendelian heredity and the spectacular developments of single-gene genetics, when others began attempting to put a single-gene, dominant-recessive pattern to every kind of human trait from the cephalic index to the "wandering instinct," Boas made use of the new knowledge without losing his own clear view of the special problems connected with biometry. He kept on with his promising leads, 50 years and more ahead of his anthropological colleagues, who were concerned with types and with racial characterizations, not with heredity and causes, and who were still to be rocked by the impact of blood-group genetics, let alone by problems related to subtler sources of variation. Boas' ideas may have sounded outlandish to them, but the utter lack of comment his work seems to have aroused makes it hard to tell.

The difficulty of the mathematics involved doubtless kept many from trying to comprehend him. I am told that he taught a statistics course of uncompromising rigor at Columbia. An informant says that, one year, Boas discovered that none of his class knew calculus, so he digressed long enough—one hour—to teach them this subject. Herskovits writes (1953) that the course had to be taken at least twice to be "mastered." Twice seems not to have been enough, since none of his students carried on in the same vein. However, this was not because Boas did not encourage statistical uses: in "A precise criterion

of species" (1898), he urged that zoologists use statistics. Boas was simply ahead of his time.

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# *Boas Sobre Cerámica y Estratigrafía*

MANUEL GAMIO

EN 1909 cuando cursaba yo estudios de antropología y arqueología en el Museo Nacional de México, conocí al Dr. Franz Boas, quien vino temporalmente a este país después de ser nombrado Director de la Escuela Internacional de Arqueología y Etnología Americanas, en la cual fuí discípulo de él. Posteriormente se me concedió una beca en Columbia University donde seguí recibiendo sus sabias enseñanzas y me gradué como Maestro en Artes y Doctor en Filosfía.

Poco es lo que podría agregar a lo mucho que se ha escrito en honor de ese gran hombre de ciencia, que tan excepcional labor llevó a cabo en lo referente a antropología y conocimientos anexos, pero sí voy a referirme a dos de las trascendentes actividades que desarrolló en México y en las cuales tuve el honor de colaborar.

Hasta iniciarse el primer cuarto de este siglo, muy pocas investigaciones propiamente arqueológicas se habían hecho aquí. Las fuentes históricas hacían referencia a numerosos grupos indígenas que habían vivido en el Valle de México, de los que si bien se sabía los idiomas que hablaron, en cambio se ignoraban las características culturales que los distinguían; tales grupos eran los llamados teotihuacanos, toltecas, aztecas, otomíes, tezcocanos, tepanecas, etc.

En 1908–1909 exploré algunos monumentos de dicho Valle y colecté las vasijas y fragmentos de cerámica que allí encontré, titulando ese trabajo "Restos de la Cultura Tepaneca" (1909), no obstante que no era justificada tal denominación.

Cuando Boas vino a México, discutimos amplia y frecuentemente sobre la conveniencia de determinar cuáles eran en realidad las características distintivas de cultura material arqueológica en el Valle de México, comenzando por la cerámica, investigación que me encargó hacer el Dr. Boas, pues entonces era yo alumno de la Escuela Internacional de Arqueología y Etnología Americanas. Entonces fué cuando después de detenido estudio, logré establecer que había tres tipos principales de cerámica arqueológica en el Valle de México y eran los correspondientes a teotihuacanos, arcaicos y aztecas. Boas a su vez, reunió una gran colección de fragmentos de cerámica azteca que fueron reproducidos en un interesante opúsculo al que se adjuntó posteriormente la descripción que de ellos hice titulada "Texto para el Album de Colecciones Arqueológicas Seleccionadas y Arregladas por el Dr. Franz Boas" (1921).

Terminada esa labor, el Dr. Boas me preguntó cuál era la respectiva antigüedad de esas tres culturas, a lo cual no pude responder, por lo que me encomendó investigarla haciendo varias excavaciones arqueológico-estratigráficas que fueron las primeras efectuadas en México. Como resultado de esos trabajos,

se pudo deducir que la cultura arcaica fué la más antigua del Valle, pues sus vestigios aparecían en los estratos más profundos; seguía en antigüedad la cultura teotihuacana, cuyos vestigios estaban en estratos inmediatos, y por último, la más moderna era la cultura azteca, que aparecía en los estratos más cercanos a la superficie de la tierra.

Entonces se llegó también a la conclusión de que probablemente la cerámica es la mejor guía con que se cuenta para identificar y diferenciar en principio a las culturas arqueológicas.

Para terminar estas líneas, me es grato expresar aquí mi profunda gratitud por la amistad y el interés con que me distinguió mi querido maestro, el Dr. Franz Boas, cuando iniciaba yo mis estudios de antropología.

# *Folklore*

MELVILLE JACOBS

FRANZ BOAS' productivity in folklore probably exceeded his remarkable output in linguistics or in physical anthropology. Appraisal of his work in some fields is easier today than when it was published, because developments in method and theory offer criteria in addition to those which his contemporaries accepted. But, since advances in method and theory in folklore have been few, judgment of his achievement in this field must rest largely upon two bases: the quantity and quality of his work compared with folklorists who preceded him or were his contemporaries; and the worth of his endeavors in the light of standards of scientific inquiry. I shall emphasize the latter.

I propose to arrange Boas' work in folklore in the following manner: field researches and text publications; teaching and support of researches by others; editor of publications; interpreter of oral literatures; contributor to method; and contributor to theory.

## FIELD RESEARCHES AND TEXT PUBLICATIONS

Among those who worked in non-European folklore, Boas was foremost in collecting and encouraging the gathering of folktales; in setting up rigorous criteria for the ways in which they should be obtained; in their use to show diffusion; and in noting the elements of social organization and culture they contained.

His earliest important scientific publications were ethnographic and folkloristic reports of field researches. The emphasis was upon careful collection and accurate reporting of "facts."

In 1884, 1885, and 1888 he printed a few folktales which he had collected among Canadian Eskimos. Some were in German translation, others in English; he included the latter in his description of the Central Eskimo (1888a). In 1888 he published, again in German, myths of Tlingit and other Pacific Northwest peoples (1888b). He quickly followed these writings and his still unpublished Northwest Coast collecting of the late 1880's with his first paper on method in folklore (1891).

A few Kutenai tales appeared in 1891, various Oregon and Washington Indian stories in 1893, and in 1894 his first volume of texts and translations, *Chinook Texts*, obtained in translation through the medium of Chinook jargon—a noteworthy feat (1894). The Chinook monograph followed the publication format which James O. Dorsey and Albert S. Gatschet had recently employed (Dorsey 1890; Gatschet 1890). Like Dorsey's Cegiha, *Chinook Texts* was a fine effort for its time, especially as linguistic source material.

Most of Boas' subsequent text publications adhered closely to the format of *Chinook Texts*, and to its dimensions: he was not interested in obtaining a large sample, much less the whole of an oral literature. He was concerned with sup-

plying linguistic materials which were sufficiently varied to document the study of the language. Indeed, Boas' first love was linguistic analysis. The number of myths or tales obtained on a field trip of two or three months was 18 for coastal Washington Chinook in 1890–1, about 20 for Kathlamet Chinook in 1890 and 1894, 23 in Tsimshian in 1894, about 23 in Bella Bella Kwakiutl in 1897 and 1923, and so on. Each session of recording included a small number of ethnographic texts and a varying amount of linguistic forms. In the Pacific Northwest a collector cannot often obtain more than 20 translated texts during a period of two or three months. Such sampling of an oral literature does not permit a full analysis of its content and style in a community which, at an earlier time, transmitted at least a hundred stories of several kinds.

Boas early became aware of the limitations in short field sessions, and so he recruited a few literate natives and also encouraged anthropological linguists to obtain folktales. Natives who could write texts might provide material superior to that dictated; the latter was sometimes untrustworthy and stylistically uneven. Boas' publications of manuscripts sent to him by the Kwakiutl, George Hunt, contributed a large segment of the oral literature which that people possessed.

A popularly oriented publication appeared in 1895 when Boas printed a book of German translations of myths which he obtained from various groups along the Northwest Coast (1895). He believed that such a sampling was also worthwhile for scientific purposes, because he chose the stories to exhibit regionwide dissemination of myth components in order to document his criticism of evolutionist and interpretation-of-nature theorists in folklore.

The task which he set for himself in folklore—to get the field research done, to publish it accurately, and to bring together evidence to display dissemination and amalgamation—was time-consuming. His range of interests and his commitment to publishing a variety of source materials stood in the way of his doing more in folklore. He worked rather reflectively in linguistics, physical anthropology, and plastic and graphic art. But even before 1900 he seems to have habituated himself to long periods of emotional release by preparing texts for publication and noting folkloristic content which had patently diffused and combined.

The year 1898 saw his publication of myths from the Bella Coola of British Columbia (1898b). In 1901, Kathlamet Chinook texts appeared—he had used Chinook jargon for the translations, as in the coastal Chinook dialect (1901); the next year Tsimshian texts came out (1902); there was a volume of Kwakiutl texts in 1905, another in 1906, and another in 1910 (Boas 1905, 1906, 1910). A second volume of Tsimshian texts was printed in 1912 (Boas 1912). Boas' thinking on folklore method and theory was virtually complete by the first World War, and its major exemplification appeared in 1916 in the prodigious *Tsimshian Mythology* which had perhaps been finished as early as 1910 (1916b).

In 1918 *Kutenai Tales* appeared (Boas 1918). The volume demonstrates Boas' two chief concerns in collecting and publishing: making available source

materials in both folklore and linguistics. The Kutenai pages include Alexander F. Chamberlain's texts, a few of which are ethnographic, obtained in 1891, in addition to a collection which Boas acquired in the field in 1915. He appended abstracts of all known Kutenai stories, with citations of similar components among a large number of North American Indian groups. The volume closed with Kutenai-English and English-Kutenai vocabularies, each item painstakingly referenced to its appearance in the texts. In summary, Boas printed all his Kutenai folklore materials without discussions of content or style, but he offered abstracts, comparative notes, and vocabularies. Such archivist contributions served principally linguists and reconstructionist comparativists in folklore, but it was a fitting decision at a time when scholars lacked theories with which to justify other ways of arranging folklore materials.

Later and larger text publications were from Kwakiutl (1921), Keresan in the Pueblo Southwest (1925b), Bella Bella Kwakiutl (1928), and his Kwakiutl group again (1930a, 1935a, 1935b). The Keresan work is of special interest both because it is his only Southwest field research and because he wrote its Indian transcription in longhand in order to circumvent letterpress printing costs.

He left small amounts of unpublished texts, among them Tillamook Salish of 1890 and Chehalis Salish of 1927. Although Salish linguistics and folklore interested him throughout his career, and he had early published some texts from Salish groups and a Bella Coola volume, he later regretted that he had not completed preparation of the linguistic and folklore materials which he had accumulated among Salish peoples of Oregon and western Washington. If he had published such data in addition to the Chinook monographs from that region, anthropologists might not so often regard him as primarily absorbed in northerly Northwest Coast and Kwakiutl archivism. His areal research interests had actually ranged from Tillamook south of the Columbia River to Tlingit in Alaska.

## TEACHING AND SUPPORT OF FOLKLORE RESEARCHES BY OTHERS

No professor matched Boas' efforts to produce fieldworkers in folklore and to support their researches and publications. He taught and influenced many people, who introjected his standards in research and publication. He often procured financial aid for their work. A remarkable quantity of folklore, as well as ethnographic and linguistic, publication was made possible by his teaching, example, enthusiasm, and fund-raising. Huge amounts of manuscripts remain unpublished because no one will now prod their owners into working them up for press. Perhaps no one has the Midas touch to finance the researches and text publications which characterized the forty or more years when Boas led American Indian folklore studies.

For years Boas gave university courses on the mathematical tools for description of populations, on the science of language, and on American Indian languages, but to my knowledge he never offered a course on folklore, though by 1910 he had set a commanding example by his publications. His use of probably diffused "elements" as evidence against current theories of folklore

and anthropology persuaded graduate students that collecting folklore was of prime importance. Students translated his strictures on incorrect theories into contributions to theory in folklore. Students who learned from him how to record glottalized lateral and velar consonants presently obtained texts during field researches; some of the others who had difficulty in distinguishing such consonants obtained texts anyway and, to be sure, the texts were excellent for the purposes of folklore science. Boas did not discourage the latter students, for he supposed that portions of their field notes would be usable and that they would develop into better workmen on later field expeditions. Indeed, only a few students returned without folklore in some form.

His impact as a teacher can be shown by comparing him with European professors such as Haddon, Malinowski, Radcliffe-Brown, and Schmidt, who also had many students but who did not transmit the value of gathering folklore. Furthermore, when his students became professors, they also encouraged fieldworkers to bring home notebooks full of myths and tales. A few of the pupils in turn have continued to inspire the collection of stories.

Students or disciples who published folktales in goodly quantity include A. L. Kroeber, Wm. Jones, L. J. Frachtenberg, E. Sapir, G. A. Dorsey, E. C. Parsons, L. Farrand, L. Bloomfield, P. E. Goddard, J. A. Mason, F. Speck, R. H. Lowie, P. Radin, F.-C. Cole, M. W. Beckwith, R. F. Benedict, M. J. Herskovits, G. A. Reichard, R. L. Bunzel, G. Wagner, M. J. Andrade, T. Adamson, G. Weltfish, E. Deloria, A. Phinney, and M. Jacobs. Most of the work was on Indians of North America, but a few scholars collected elsewhere: Cole in the Philippines, Beckwith in Hawaii, Herskovits in Suriname and Dahomey, Andrade and Parsons in Latin America. The inventory of publications is spectacular, the more so upon comparison with the modest quantities which English and continental contemporaries brought back from non-European peoples.

Except at two or three universities, Boas' influence had attenuated at the time of his retirement in the late 1930's. American linguists and anthropologists had turned to new interests, and few felt the need to record, publish, or compare folklore archive material. Editors of anthropological series were no longer receptive to collections of texts or to tales printed in English alone. A half century of Americanist archivism, the creation of one man, had quickly withered. Only one or two students, such as Beckwith, had specialized in folklore; the others were primarily linguists or sociocultural anthropologists.

## EDITOR OF PUBLICATIONS IN FOLKLORE

Boas' herculean labors as an editor of technical publications competed with the hours which he spent examining doctoral dissertations and manuscripts which pupils and disciples placed on his desk. He was not indifferent to English punctuation and style, but his standards in such matters were not severe. The speed with which he edited and proofread resulted in many printing errors. His principal criticisms of manuscripts were directed to method, content, and arrangement. His concern was to advance the frontiers of knowledge as he under-

stood it—to make descriptive and comparative notes available in print, and to censor speculations which he regarded as unsupported by evidence.

His editing of folklore included monographs in the Jesup North Pacific Expedition Publications, which came out between 1900 and 1930.

Sixteen of the 17 Publications of the American Ethnological Society from 1907 to 1937 were Boas' kind of folklore text-and-translation volumes. He used the Society almost entirely for publication of folklore texts. I do not know if he rejected other kinds of manuscripts when authors submitted them to the Society, but more diversity of publication would have strengthened the organization by expanding the numbers of social scientists who were interested in it. Editors who succeeded him favored grammatical, ethnographic, and other types of inquiry. The Society's Monograph series, which commenced in 1940 and has printed more than 20 items, contains only one which deals with folklore. It is as if the Society voted almost 20 to one in favor of folklore when Boas was its mentor, and better than 20 to one against in the two decades since: the sole publication is an interpretive treatment by a psychoanalyst (Roheim1954). When Boas was editor he probably would not have accepted such a work. His dislike of psychoanalytic hypotheses, and of what he considered their hypnotic pyramiding, was as thoroughgoing as his repudiation of interpretation-of-nature theorization. No doubt few authors submitted manuscripts containing views of which he was known to disapprove.

Boas bore a heavy burden from 1908 to 1924, when he was editor of the Journal of American Folk-Lore. He processed at least seven of its Memoirs, but the three by his devoted disciple, Elsie Clews Parsons, were doubtless prepared by her. One or more Memoirs of later years required lengthy editing, notably the Adamson collection of Washington Coast Salish folktales (Adamson 1934).

From 1910 to about 1936 Boas edited the Columbia University Contributions to Anthropology. The 26 or 27 issues of that period included a group of five, from 1910 to 1928, which were of the same sort as the texts-and-translations of other series. Another seven had appeared by 1935. Benedict's *Zuni Mythology* (1935) was in English, and the Herskovits' *Suriname Folk-lore* (1936) had varied content in addition to texts. Fourteen of the 27 publications had been folklore, and 13 of these were also linguistic sources. Again, after Boas' retirement, the Columbia University series offered nothing in folklore.

Boas employed the International Journal of American Linguistics, which he initiated in 1917 and published intermittently until 1939, to make available several publications of texts. Four Alsea texts, recorded by Frachtenberg and deleted from his forthcoming Bureau of American Ethnology Bulletin (Frachtenberg 1920) because of their sexual content, appeared in the International Journal (Frachtenberg 1917). Boas' Puritanism never stood in the way of getting dictations printed, even if, as in his earlier text volumes, he had to disguise a translation by putting it into Latin. Boas also placed in the Journal Mason's *Tepecano Prayers*, Speck's *Penobscot Transformer Tales*, and Goddard's *Wailaki Texts* (Mason 1918; Speck 1918; Goddard 1923). When he in-

serted texts in the pages of a linguistic journal he appeared to be indicating that the principal value of any texts was linguistic. However, he would have asserted that folklore texts had double utility and that the important thing was to publish them wherever one could, even in a periodical of tiny circulation.

## INTERPRETER OF ORAL LITERATURES

Although Boas and his followers salvaged samples of scores of oral literatures, their linguistically oriented and insufficiently annotated publications prevented appreciation of literary or artistic qualities in them. Boas appears not to have envisaged the possibility of a contribution to the archives of literary art, although a few of his former students, such as Paul Radin, both perceived it and tried to implement it. Boas was satisfied to direct his work to linguists, folklorists, and anthropologists, not to literary critics or a literature-oriented public.

Can both scientific and literary functions be combined in publication of folktales and still adhere to rigorous standards? I believe that they can, if interpretive comments supplement the stark translations and if the additions are written so as to include, readably and pleasurably, associations and sentiments which the native audience experienced.

Boasian collections of folktales remain almost unknown to the general public because their sociocultural background has not been made intelligible and because they lack pleasurable readability. In order to reach that public, a few Boasian folklorists have written explanatory prefaces and have translated freely; an excellent example is *Californian Indian Nights Entertainments*, by E. W. Gifford and G. H. Block (1930). But the background is spare and stylistic features almost disappear in such works. They are neither useful to science nor esthetically close to the native original. Principally, they allow the reader to become acquainted with manifest rather than latent plot content and not at all with literary form. Like most of those whom he brought into folklore, Boas himself was disinclined to devise a new way of presenting folktales to the nonprofessional.

## CONTRIBUTOR TO METHOD IN FOLKLORE

Theory determines method, and method leads to the relation of evidence to theory. Because theory and method interconnect, they require integrated treatment. But proper evaluation of Boas' work in folklore dictates a discussion, first, of his emphasis upon some principles of descriptivist method.

Boas responded creatively to 19th-century folklore collecting and theorizing. His reaction took two principal forms: discussions of methodological requirements for tenable statements about origins of elements; and explanations and exemplifications of methods needed to minimize errors or distortion in collecting and publishing source materials.

His earliest writing on method dismissed, as incapable of proof, theories about origins of plots and elements (1891). Instead, he suggested the need to study their history. He explained that since similar plots or motifs demonstrably occur in continuous areas, the only inference is historical: the plots or

motifs diffused. He said that folklore elements had combined in different ways in different areas. He never attempted to discover, discuss, or contrast the kinds of elements suitable for meaningful comparative researches. He accepted familiar constructs of motifs, actors, themes, explanatory elements, and the like. These were his elements. He cited indications of their recombinations in order to combat the theory that similar elements originated independently many times. He did mention the need to study the psychology of dissemination and amalgamation, but he did not pursue the implications of this statement.

His 1891 paper contains the gist of most of his later methodological writing: collect many folktales from contiguous peoples and plot the distribution of elements to exhibit their various combinations. Theoretical work may then be possible. He never suggested when collecting, mapping, and hypothetical historical reconstruction had been sufficient to justify entertaining theory.

What did he mean by theory in folklore? He was never explicit in writing, but it is clear from his conversations that by the 1920's he meant statements of a psychological kind. If consistent with deductions about historical processes, these would also point to the social and cultural life of the people and would thereby account for borrowed elements, for changes after borrowing, and for recombinations. In the 1920's when he discussed anthropology with me, he regarded psychological statements as basic, but his writings show that he preferred statements about historical processes. The decision which he made arose from his inability to interest himself in or to accept more than a small portion of any current system of psychological theory.

Although cross-cultural sociology was to be found at Columbia University in the 1920's, Boas never conceived of the possibility of weaving sociological formulations into folklore. He remained an historical reconstructionist because he rejected psychological and sociological theories.

From 1890 to 1920 Boas often offered succinct statements about field method and comparative study of folklore elements: to him these were aspects of a general topic of method. In 1890 only a few important collections of American Indian folktales had been obtained by the phonetic text-and-translation method. Boas had mastered it and was turning out qualitatively unexcelled manuscripts. The seeming exactness of this method, exhibited in the Chinook, Kathlamet, Tsimshian and other early texts, overawed most anthropologists and folklorists until Sapir and others consistently produced equally fine and even superior results, at least for linguistic science (e.g. Sapir 1909; Bloomfield 1928). By the 1930's it was Boas' quantity of production rather than his method which remained preeminent.

Boas' method was fundamentally the same as the method of historiography since Ranke: make available every item obtained, in a form which corresponds as closely as possible to reality. Such a stress was important at a time when oral literatures were frequently published in a Europeanized literary style, without care for native content and form.

Boas' best writing on method, *Mythology and Folk-Tales of the North American Indians* (1914), climaxed a generation of industrious collecting and fol-

lowed completion of his manuscript of the *Tsimshian Mythology* (1916). The latter was a combined study of Tsimshian myths and of folktale element distributions and combinations in northwestern North America and also contained material on features of Tsimshian life mentioned in the folktales. The 1914 paper was actually a statement of the methodology underlying *Tsimshian Mythology*; it accented "demands for accuracy of record"; notations of alternate versions; determinations of the geographical point of origin of each folktale; presentation of native text and close translation rather than a version collected in a European language (especially for study of literary form); and, wherever possible, employment of literate natives to write their own folktales: Boas had enjoined a Kwakiutl and a Tsimshian to send texts to him. He asserted that it might be well to record oral literature on phonograph records, and he urged that the folklorist not limit his work to the "right" or "best" informant, or the "right" version, in groups where variants occurred and where such versions affected transmission of the literature. In 1920 and later, he emphasized the need to collect American Indian materials which contained Negro slave, French Canadian, Spanish, and Portuguese borrowings, in order to study processes of assimilation and adaptation (1920). The injunction was relatively new, and many disciples (e.g. Parsons) presently interested themselves in such studies.

In his 1914 paper Boas again showed that folktales diffuse, and he cited the familiar criteria for proving diffusion or independent origin. His repeated instructions on the diffusion of elements, and on the importance of mapping them to determine their history, contrast with his rather late and always minor interest in diffusion of linguistic features and with his lifelong insistence on an initial structural analysis of a language before undertaking comparative or historical linguistic work. In folklore his method was primarily diffusionist and historical; in linguistics it was structural. Since he never compared the premises of these opposing procedures, he did not see that his method in linguistics might advance knowledge of process, and that his method in folklore precluded it.

His bulkiest volume, *Tsimshian Mythology*, appeared a little more than 30 years after his first notations of folktales. This monumental study of element distribution had resulted from the need which he felt, as early as the 1890's, to develop a method that would adduce conclusive evidence against incorrect theories on the origins of folklore.

Boas' policies and habits of work in the field were shaped not only by rational considerations but by the ways he felt about people and by the relationships he established with them. When he obtained texts he assumed that his informant was a mouthpiece for a sociocultural heritage, and therefore that folktale recitals must be recorded verbatim, without questions which might alter the dictation or lead to discussion of the raconteur's feelings or background. Obscenity was too distasteful to permit him to make inquiries about sexual components of stories. One cannot suppose that he enjoyed slang or primitive music; he felt that both were rather crude. Although he had high regard for primitive plastic and graphic art, he did not accept the equal worth of folktales and primitive music. However, his response to their esthetic char-

acteristics, his displeasure with their content and forms, did not lessen the obligation to make accurate recordings. Because he depreciated their esthetic values without being wholly aware of it, he had no interest in discussing natives' sentiments about them. He knew that all peoples cherished their arts, but he did not elect to find out why; it was enough to report, with utmost fidelity, upon the forms which natives expressed. A basic factor in his admonition to obtain mechanically perfect recordings may be found in his resistance to identification with people. The austere visitor probably mingled politely with the natives, but with some discomfort and always with a feeling of pressure to get the scientific task accomplished. Participant observation was much less possible for him than for most of his students. His temperament would not have permitted him to devise a method of analysis which dwelt upon people's feelings about their myths and tales. Nevertheless, the art form itself must be recorded, and with the greatest accuracy.

When he was nearly 70, Boas assembled his ideas about the arts in the widely read book, *Primitive Art* (1927). Unlike those who are interested entirely in plastic and graphic forms, Boas believed that art includes verbal, musical, and dance expressions and that a work on primitive art must contain treatments of all the arts. He did not compare their importance and social functions, and his book highlighted plastic and graphic art. Again, he discussed the traditionally listed components of folklore, principally in order to counter incorrect theories of origin; but he did not absorb himself, as he had earlier done so thoroughly in folklore, in regional comparisons of elements. His interest in carved designs and needle cases, for example, had developed after an immersion in comparative folklore. Thus he did for plastic and graphic art essentially what he had done for sound units and grammatical elements of the languages on which he had worked in the 1890's: he utilized a number of minimal units in the analysis of a few plastic and graphic arts. He was therefore able to demonstrate how a native selected from an inventory of units of expressive design, and he could point to material resource, technical, and other factors which caused or maintained such selection.

By the 1920's Boas had been surpassed by no one in the success of his techniques in language and plastic-graphic art. In both, the method involved the scientist's selection and arrangement of minimal units—sounds, grammatical features, or elements of technique and design. In design the method permitted disclosure of technical and social factors and of the reasons for the artist's choice of some items and rejection of others. He never applied an analogous componential-structural method of analysis to oral literature, which he had studied since the 1890's in terms of mapping gross segments of plots.

We must inquire further why so outstanding a pioneer in structuralism never carried it over into folklore. Why did he not see that his elements were too complex to permit meaningful analysis of content? Why did he not identify more than a few of the most obvious units of literary style?

A principal reason lies in his failure to formulate the essence of his methods. Although he worked with units in language and plastic-graphic art, he did not really perceive what he was doing. Few social scientists of the time understood

the import of 18th and 19th century discoveries of significant units and categories in the physical and biological sciences. Boas was no more able than others to apply, deliberately, certain methods of analysis which had been useful in more mature sciences, though he had hit upon them in language and visible arts. His earliest labor on oral literatures and on their shared features had been premised on what he and other folklorists called "elements." Their comparison had been effective in criticism of untenable theories. Had he sought smaller units of folklore content he might have been led to formulate psychological statements—such as those about displacement and projection—which were Freudian and therefore intolerably speculative. Nevertheless, folklore studies could not move toward the very maturity which Boas' work had made possible in linguistics and plastic-graphic art until identification and classification of proper units of content and style had commenced.

## CONTRIBUTOR TO THEORY IN FOLKLORE

Boas paralleled his contemporaries in his failure to offer important suggestions for theoretical advance in folklore. The prodigality of his writings, the accumulation of quantities of uninterpreted texts and translations, and the never-ending diffusionist emphasis appear to have inhibited theoretical inquiry among his students who became folklorists. To be sure, European folklorists had similar preoccupations; they too diminished or neglected inquiry of a theoretical kind. By the 1930's, European and Boasian folklore seemed so sterile that, like physical anthropology, it was all but forgotten in the growth of theory in the behavioral sciences. In spite of the invaluable criticisms of speculations about ultimate origins and the presentation of evidence for diffusion, it appeared to some that Boas' massive accomplishment had consisted of little more than an addition to factual knowledge of the stories told in many American Indian groups.

Whatever the causes may have been for their self-assurance—the untheoretical writings of Boas and his followers were perhaps a factor—uncritical Freudian, Jungian, and other psychological theoreticians entered the field after the First World War. They were not professional folklorists. No maturing science founding its own structure of theory was available to discredit them, while they ignored cardinal principles of method which Boas and others had established. Folklore lacked writers of a critical bent who could effectively counter absurdities in the amateurish theorization which sometimes called itself folklore.

Boas had been in the vanguard in exposing deficiencies in method and theory which folklore publications contained before the 1920's. But the indefatigable collector and his many students and followers, who were the most tireless gatherers, cataloguers, and cartographers of folklore items outside Europe, had not pointed out the way toward a theoretical structure of meaningful units, processes, and relationships which were expressed in oral literature. Twenty years after Boas penned his last statement on folklore (1938), the discipline remains almost exactly where he left it: an immature science which possesses a towering mass of essentially incomprehensible miscellanea,

most of them ticketed according to geographical provenience. A modest shelf of diffusionist theses stands beside the descriptive items.

Regrets about the present state of folklore as a science must not function as depreciation of the value and timeliness of Boas' statements of method and his criticisms of theories in folklore: potentially they cleared the air and they prevented his disciples from talking theoretical nonsense. On the other hand, many folklorists and amateurs who entered the field from the disciplines of literature and psychiatry never encountered Boas' writings or they failed to perceive the significance of what he had said. Let us glance at his theoretical work.

From 1891 to 1938 he repeatedly urged that current theories about plot, motif, and actor origins could in most instances be subjected neither to proof nor disproof, and that advocates of Pan-Aryan and Pan-Babylonian diffusions of myth content were wrong. He countered with North American evidence of dissemination which showed that most folklore components had been borrowed from nearby peoples and then adapted. For example, he wrote in 1896, "The analysis of one definite mythology of North America shows that in it are embodied elements from all over the continent, the greater number belonging to neighboring districts . . . " (1896). In this paper, as in later ones, he explained that the prehistory of each folktale is complex and may contain some response to natural phenomena—as suggested by Ehrenreich and Preuss—but usually such a response is only a small part of its development. He added, "Wherever geographical continuity of the area of distribution . . . is found, the laws of probability exclude the theory that in this continuous area the complex phenomenon has arisen independently in various places, but compel us to assume that in its present complex form its distribution is due to dissemination, while its composing elements may have originated here and there. . . . " Such statements exemplify Boas' memorable contributions toward discrediting popular but improbable theories.

Others of Boas' critical observations are best included in the following comments on his constructive work in folklore theory. His seminal contributions to such theory were scant, and contrast with his trenchant criticisms of errors in theory. He indicated, in general terms, only a few directions in which factors of causation and development might be sought, and he had almost nothing to offer toward a theory of style.

In 1916 he said, as he did before and after, that the origins of folktale elements are explicable in terms of "the play of imagination with the events of human life . . . particularly those that stir the emotions of the people" (1916b: 880). For the notion that myths arise from contemplation of nature, he substituted the thought that they most frequently arise in "the social life of the people." Although he never sought proof—nor the means of attaining such proof—for his common sense formulation about origins in "events of human life" and "social life," he deserves credit for reiterated insistence upon it.

In one of his last writings he added a few simple, unspecific, and undocumented suggestions to his statements about processes of literary creativity (1938). In addition to exaggeration, "play of imagination" and "events of

social life," he cited day-dreaming, wish fulfillment, and turning experiences into their opposites. However, he did not indicate the frequencies of such processes, or the plots, motifs, and actors that had developed in terms of them.

In the late 1920's he wrote, "Since the tale is an artistic unfolding of the happenings in human society, it must reflect the habits and conflicts of life of the society in which the narrator lives. Themes like the conflict between father-in-law and son-in-law, or between stepmother and stepchild, must be considered from this angle . . . " (1930b:93). He did not suggest why oral literatures contain some but not all of the relationships which appear in community life.

In *Tsimshian Mythology* he said, "It is obvious that in the tales of a people those incidents of the everyday life that are of importance to them will appear either incidentally or as the basis of a plot . . . " (1916b:393). Neither he nor other folklorists had either perceived or demonstrated that the content of myths and tales included projections of points of stress in the society; that is, events and social relationships portrayed in an oral literature connected with relationships which were unsatisfactorily resolved by social structure and custom. To be sure, folktales express a great many incidents and relationships, but only a few are stressed. Failure to make this distinction is a deficiency in discussions about the processes which determine expression of content.

In *Tsimshian Mythology* Boas briefly discussed borrowing and adaptation (1916b:571), and pointed out that the multiplicity of incidents in Raven myths suggests a tendency to incorporate any tale or incident that "fits." Therefore, he indicated, a community adds independent shorter stories to its stock of longer ones; but he did not suggest the processes which determine why only some items are felt as suitable for amalgamation.

In an early writing which was especially rich in constructive suggestions of a general kind, Boas pointed out that the content of an oral literature is due both to independent invention and to borrowing and adaptation, "under the influences of environment and of new social conditions," and that the extent to which the content is explicable by one or the other factor "remains to be determined by detailed comparative studies" (1898b). He asserted that each society is constantly remodeling borrowed myth content and does so in terms of social structure and religion. "Both factors, dissemination and modification on account of social causes, must tend to obscure the original significance of the myth."

Again, in *Tsimshian Mythology* he wrote that "every-day wishes created reality" in mythic forms; every-day experiences were subjected to exaggeration (1916b:880). He emphasized processes such as imagination, exaggeration, and wishes. The purpose of his book was not to specify processes of origin and development of content but rather to point to the borrowings and recombinations which might exhibit such processes. Indeed, an appropriate, if cumbersome, title could have been *The distribution and recombining of plots and incidents in Pacific Northwest folklore*, with a subtitle such as *Reflections of Tsimshian culture in the mythology.*

Boas wrote several times about another feature of oral literature, the inclusion of explanatory items and their historically "secondary" role. He did not point to specific factors in the invention, borrowing, manner of manipulation, or functions of explanatory elements because his central aim was to refute assertions about their historical primacy. Boas and his followers demonstrated that explanatory elements were prevailingly "secondary additions," just as he and his students offered documentation of the usually secondary role of design elements in plastic and graphic art. Since he did not differentiate content and style, he did not point to the predominantly stylistic function of explanatory units.

A generalization about causation and development appeared in several papers: in wealthier societies, specialists such as poets and priests render an oral literature more complex and rework it systematically (1914). He suggested no procedures for revealing the processes of creativity characteristic of specialists. He felt that systematization, which was in the hands of priestly specialists in wealthier societies, was of minor import in "poor" societies. Dissemination indicated to him that people tend, especially in the absence of specialists, to borrow freely some items which contradict portions of their repertoire of myth elements. Therefore, material of heterogeneous origin is included in what he designated as a loosely-connected whole. To be sure, the words "freely" and "loosely" begged important questions. Boas' point of view about the structuring and content of an oral literature made it a thing of shreds and patches. His attitude appears in his review of a book by G. W. Löcher, who regarded an oral literature as a system or structure (1933). Boas demurred, asserting that a folklore is an assemblage of items of which many continue to be contradictory. Although he was correct to the degree that his considerations dealt with some of the borrowed items, his patchwork assumption prevented him and most of his disciples from seeking or accounting for such structuring and internal consistencies as were present.

Two books, *Tsimshian Mythology* and *Kwakiutl Culture as Reflected in Mythology* (1935b), and several shorter writings document the formulation that the essential features of a society and culture "appear distinctly mirrored" in their folktales. Only once did Boas state that an oral literature "mirrors" some but not all of the way of life: in this instance he wrote that secret societies and other aspects of Tsimshian life occupy an unimportant part in the myths and tales. In order to display what he regarded as a kind of mirror effect, he catalogued technological and sociocultural features of Tsimshian and Kwakiutl life to which their folktales refer, and indicated some features which recur in the stories. Since he did not count recurrent features, it is impossible to weigh their significance. He did not explain presence, frequency, and absence of features. His contribution amounted to an index which is as useful to folklore theory as a dictionary to the presentation of a theory of grammar.

In a number of places he mentioned that European tales express a way of life of earlier centuries, while among other peoples their present manner of living appears in the tales.

Boas several times offered a distinction between North American Indian myths and tales: he correctly rejected definitions based on the "subjective standpoint" of an outsider, and said that categorizations must be made on the basis of the premises by which the natives group and contrast stories (1916b: 565). He showed that supernatural and other "mythic" features are present in both myths and tales. Although these views are salutary, he did not know that some Northwest communities categorized their folktales in three or more types. In fact, Boas' fieldwork did not always establish a narrative as myth or tale; in some instances he appears to have neglected to inquire how natives classified their stories.

He categorized North American Indian plots as trickster, transformer, human, and animal, because of the names and some of the characteristics of leading actors (1914). This classification is no more efficient than a taxonomy which captions languages as isolating, inflecting, polysynthetic, and agglutinative. Many stories exhibit features of several or all his types, just as most languages contain some or all of the features of the four language types.

Boas often used the concept of a cycle for a number of "loosely connected" Coyote stories of the Plateau. He said that the grouping of stories around one culture hero produced a cycle. Actually, the concepts of cycle, culture hero, or mere hero are improper in content or style analysis in most of the Northwest. Nothing really groups Plateau Coyote stories except the personality features which Coyote actors shared: Coyotes were often only people of a special kind who had lived in the land before the modern period.

Boas oversimplified when he called Coyotes, or other leading actors, transformers or tricksters. Each such actor has a variety of characteristics, only some of which are transforming and tricking. Announcing the future, as both leading and minor actors do, is equally important over much of the Northwest. Concepts such as transformer, trickster, and culture hero are stereotypes which do not serviceably segregate plots or actors in many Northwest literatures. When Boas employed these notions he went against his principle that the anthropologist must take care lest he improperly read in concepts from another cultural heritage.

Boas' theoretical work was perhaps widest of the mark when he discussed causes of the personality traits ascribed to Northwest actors. In 1938 he said, "the most important characteristic of mythological concepts is personification . . . Personified animals appear everywhere in tales. In striking contrast to the human actors, they are sharply characterized according to their observed habits . . . " (1938:614–5). In 1914 he had written that origin and trickster myths "deal with types that are either so impersonal that they do not represent any individual, or are merely the personification of greed, amorousness, or silly ambition. Wherever there is individuality of character, it is rather the expression of the apparent nature of the personified animal, not the character that fits particularly well into human society" (1914). The deficiency in such theorizing derives from an inability, shared by fellow folklorists, to see that animal characteristics of actors arise in at least two ways: as defensive

devices, and in identifications of actors with animal spirit-powers. The actors' characteristics resemble or, by displacement, veil those human traits which are really being projected into the actors. In the Northwest, animal actors are disguised or caricatured human beings. They are people who, projected onto the fantasy screen, stand for and relieve points of tension in society. For example, immature and sadistic youths—such as some Coyote actors—may be depicted as engaged in tricking; headmen, well-to-do, or other persons who exert controls within villages are often represented as engaged in transformations of nature. These and other folktale actors sometimes had animal names and aspects because of associations of persons with animal-like supernaturals. Persons identified with their spirit-power kin. Throughout Boas' lifetime, the dynamics of the formation of folktale actors were apparently beyond the understanding of folklorists. They turned the analysis upside down: animals are not the starting points for folktale actors. People are.

The processes of projection which account for vital plot and thematic content were also obscure to most Boasians. They could not identify causes of creation, manipulation, or maintenance of folktale content. For example, Boas cited but could not explain the frequent occurrence of starvation; of rivalries between clans; of elopements with supernaturals; of husband-wife love; of relationships between grandparents and grandchildren; of instances of shaming; of poor boys who rose in social status; and of authentication of privileges. Many folktale themes, more important than those he cited, escaped his notice, perhaps because they arise in poorly resolved tensions in community life rather than in manifest aspects of social organization. Such themes include hostility to elders and grandmothers, in-law tensions, and growth and development in a man.

Folklorists neglected to study closely a number of important facets of life such as world view, ethics, and humor, which a community employs in more or less structured forms in its folktales. In his Tsimshian and Kwakiutl writings Boas only inventoried a few items of such facets of a culture, although he listed all the more external traits of society and culture which he recognized in the myths. He said that "Material of this kind does not represent a systematic description of the ethnology of the people, but it has the merit of bringing out those points which are of interest to the people themselves. They present in a way an autobiography of the tribe" (1916b:393). This general statement was not pursued by study of the "points which are of interest to the people" or by study of those points which the people would not select for expression in their stories.

In a passage which contradicts Boas' usual willingness to accept evidence of creativity, he wrote, in 1938, "A study of tales . . . gives very slight evidence of the ability of man to invent new [folktale] motives. It seems rather that his imagination is restricted by the current forms [the principal themes] of tales" (1938:612). The thought that a Jungian or other protagonist of archetypes might find this passage congenial would have outraged Boas. In fact, it really controverts his opinion, expressed in the same chapter and in earlier writings,

that the contents of folktales arise in and are maintained by existing social and cultural behavior. Boas' one expression of the idea that primitive storytellers are uninventive may have resulted from his awareness of recurrences of folktale content in contiguous peoples.

Boas theorized even less about style than he did about content. He supposed that texts and translations supplied most of the source materials necessary for study of style. He did not observe that audience participation and manipulation of content in conversations outside formal recitals must be studied too, because they contained decisive factors.

He did not explicitly differentiate between content and formal features, and in all his writings he enumerated few specific characteristics of style. The list includes vocal mannerisms or special speech forms, consonant changes as in Chinook, rhythmic repetition or pattern number, repetition to provide emphasis, simile, metaphor, omissions of certain features of nature and prescribed citations of others, notations of literary forms such as novel-like and Odyssey-like plots, very short stories, proverbs, riddles, moralizing fables, and a few others. The tally is so small that it is evident that he never intensively considered style in an oral literature. Although he carefully examined manifestations of style details in Northwest sculpture, he observed only general matters of style in folklore, and these in phrases which did not develop method or theory. For example, he wrote a number of times that he regarded the "folktale primarily and fundamentally as a work of primitive art."

He directed himself mainly to criticism of unsatisfactory theories about style. He commented adversely on Bücher's and Wundt's speculations that rhythmic repetition arises in labor and the dance. He cited many examples of rhythmic repetition in myths and song words, and gave assurance that such rhythm is one of the "fundamental esthetic traits" (1927:310). In 1914 he wrote that pattern numbers develop "from the aesthetic values of rhythmic repetition. Its emotional effect is obviously inherent in the human mind . . . the difference [from region to region] in favorite rhythms may account for the occurrence of different sacred numbers . . . " (1914). He did not mention social and religious reinforcement of sacred numbers.

Boas' inattention to literary style is exposed by a statement in *Primitive Art*: "An epic diffusiveness, and insistence on details is characteristic of most free primitive narrative"—that is, folktales (1927:309). The statement is puzzling in view of the stylized selection of a few descriptive details in Northwest literatures, especially in groups south of Kwakiutl. He had early commented on the paucity of description in Eskimo tales, but in a paper which he wrote about the same time as *Primitive Art*, he recognized the Plateau's stylized omissions of descriptions of nature (1925c). In his later *General Anthropology* he admitted an "inclination to diffuse detail" in some regions and "pregnant conciseness" in others (1938:595). He had quietly corrected his error.

An absence of notice of units of style is revealed in the statement that different themes or types of plot "influence the form of the narrative, because the incidents are tied together in different ways" (1925c). He did not analyze "form of the narrative" or the ways in which incidents are tied.

Because of the commitment to exact recordings in text and translation, Boas and his followers appear to have lost sight of audience and community as loci for many of the factors which determine style.

Actually, Boas wrote meritoriously about style at a time when many decades of research had still not produced satisfactory statements about style in non-Western oral literatures. He had perceived, before other folklorists, that stylistic aspects of non-Western folktales should be studied, and said so. It needed to be said, even if nothing was done to get analyses under way. He halted after terse comments on the problem; he offered no method of solving it. During his life, only his student, Gladys A. Reichard, ventured to analyze a tribal folktale style (Reichard 1947:5–35).

Boas' writing on poetry is less suggestive, because it is not even programmatic. When Boas mentioned poetry, he said that among primitive peoples it appears only in singing (1925c:329; 1927:302). He considered the words which accompany songs to be "the foundations of poetry." In an assertion which was surprising for an anti-social-evolutionist, he added that "song is older than poetry and that poetry has gradually emancipated itself from music" (1927: 303). Again, he said in *Primitive Art*, "Poetry without music, that is to say forms of literary expression of fixed rhythmic form, are found only in civilized communities, except perhaps in chanted formulas. In simpler cultural forms [that is, in primitive societies] the music of language alone does not seem to be felt as an artistic expression" (1927:301). These lines suggest insensitivity in field observation and acceptance of a premise that rhythm is the decisive feature of poetry. Collectors of folktales generally appreciate that natives create and speak poetically either with or without rhythm, and respond to those who have spoken poetically. Boas appears not to have sensed the poetic component in folktale recitals, for he observed that folktales are more like oratory than anything else (1925c:329; 1927:303). The analogy is unproductive. He did not react to the palpable qualities and structuring of folktales which give them the character of drama.

When he wrote about riddles, proverbs, and fables, he commented solely upon their comparative or entire absence as literary forms in the New World.

The significance of Boas' constructive theoretical writing is in its exemplary emphasis upon areas in which one may seek processes that effect folktale content: dissemination, amalgamation, recombination of so-called elements, and origins in events and social life. But he did little to illuminate the way in which the processes operate or the psychological mechanisms which connect with them.

Boas' theoretical work in folklore therefore adds up to a lifetime of critical writing which was timely, and a very few suggestions toward constructive theory. Undoubtedly he carried on his studies at too early a period in the development of the behavioral sciences and of literary criticism to subject an oral literature to a micro-analysis. He therefore did not identify minimal units of content and style, and thus could not discover the sociocultural factors which created or shaped them, or the integral psychological processes. His statements and supporting evidence of process deal largely with regularities in North

American dissemination and recombination of plots and motifs. These regularities are a small segment of a general theory of folklore content, and they do not refer to style.

Do the Boas folktale collections, and the similar materials published by his disciples, contain evidence which can support a system of theory? Some of the collections are so large and so accurate, and their impact on folklore science has been so impressive, that this question should be answered carefully.

Since each oral literature is nurtured in a sociocultural milieu, it can be used for a theory about process only to the degree that its setting has been depicted. The better the ethnographic detail, the more knowledge we may gain about the processes which determined projective expressions and the forms in which they were cast.

Where ethnographic research has been slight, as in Boas' two Chinook groups, probably little can be reconstructed about the processes which determined expressions. Boas' folktale collections in those groups were also small, and so only a little can be deduced about the sociocultural life.

His partly unpublished Tillamook collection, also a small one (1898a), is supplemented by collections recorded by May M. Edel (unpublished, 1931) and Elizabeth D. Jacobs (1959). The latter, whose field work was done in 1933, also obtained ethnographic materials to add to the items published by Boas (1923). The augury is therefore more favorable than for the two Chinook peoples. Tillamook projective expressions, and the dynamics which relate to them, can probably be reconstructed to some extent.

On the other hand, Boas' ample collections from northerly coastal groups—especially Bella Coola, Tsimshian, and Vancouver Island Kwakiutl—and from a Keresan Pueblo in the Southwest, are accompanied by extensive ethnographic documentation, and there is considerable hope for success in studies of process. Kutenai, Bella Bella Kwakiutl, and some others do not seem to hold as much promise. Many of Boas' followers collected rich harvests of folktales in groups whose sociocultural life is well known, especially in the Southwest, and Boas must receive credit for encouraging and supporting their work.

The folklore theory which may develop upon the basis of studies by Boas and his disciples has a more impressive potential than those from any other source, due largely to the accuracy and quantity of materials which Boasians collected. However, the prospect is dimmed by one important consideration. No professional folklorist appears to have felt it necessary to use a field question-and-answer method—which in linguistics quickly lays bare sets of units and their arrangements. When folklorists have recorded folktales, they have only infrequently inquired about the recitalists' and auditors' responses to folktale items. Only imaginative interviewing can elicit the feelings and associations upon which folklore theory may be founded. Research may continue to be nondirective, observant, and passive during a recital, dictation, or native discussion, but if theory is to be served more than suggestively and superficially, probing inquiries must be made after the story is recorded. Folklorists henceforth should emphasize this simple requirement of method, which to a

degree has long been accepted in linguistic and ethnographic field researches, by Boas and others. In the Northwest, inquiries can no longer be made in societies such as the coastal Chinook and Tillamook, for they are now extinct as cultural heritages—and it may be already too late to return to most of the other Northwest communities in which Boas and his followers collected folktales.

They did not record the wrong things in the field. Because they did not pursue theory, they failed to collect many necessary things. Their dedication to linguistics and diffusionist method, and their lack of concern with devising fresh scientific procedures, prevented them from comprehending that a sociocultural background must be analyzed in conjunction with a close analysis of texts. Nevertheless, folklore owes much to Boas and his group for principles of method, for critical clarification of earlier theory, and for huge collections of documentary material. Folklore may build rapidly upon these foundations.

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# *Boas' View of Grammatical Meaning*

ROMAN JAKOBSON

"THE man killed the bull." Boas' glosses to this statement in his compendious outline *Language* (1938) form one of his keenest contributions to linguistic theory. "In language," Boas says, "the experience to be communicated is classified from a number of distinct aspects" (1938:127). Thus in the sentences "the man killed the bull" and "the bull killed the man," two opposite word sequences express a different experience. The "topics" (a generic term suggested by Yuen Ren Chao for the subject and object) are the same—man and bull, but the agent and patient are distributed differently.

Grammar, according to Boas, singles out, classifies, and expresses various aspects of experience, and moreover performs another important function: "it determines those aspects of each experience that *must* be expressed." Boas astutely disclosed the obligatoriness of grammatical categories as the specific feature which distinguishes them from lexical meanings:

> When we say, "The man killed the bull," we understand that a definite single man in the past killed a definite single bull. We cannot express this experience in such a way that we remain in doubt whether a definite or indefinite person or bull, one or more persons or bulls, the present or past time are meant. We have to choose between aspects and one or the other must be chosen. The obligatory aspects are expressed by means of grammatical devices (1938:132).

In our verbal communication we are faced with a set of two-choice situations. If the action reported is "kill," and "the man" and "the bull" function as agent and patient respectively, the English speaker has to select between (A) a PASSIVE and an ACTIVE construction, the first focused upon the patient, and the second upon the agent. In the latter case, the patient and, in the former, the agent may but does not have to be designated: "The man killed (the bull)" and "The bull was killed (by the man)." Since the mention of the agent in passive constructions is optional, the omission cannot be regarded as elliptical, whereas a sentence like "Was killed by the man" is a salient ellipsis. When having chosen the active construction, the speaker must, furthermore, make such binary selections as (B) PRETERIT (remote) or NON-PRETERIT: "killed" vs. "kills"; (C) PERFECT—in Otto Jespersen's interpretation (1924, 1954) retrospective, permansive, inclusive—or NON-PERFECT: "has killed" vs. "kills," "had killed" vs. "killed"; (D) PROGRESSIVE (expanded, continuative) or NON-PROGRESSIVE: "is killing" vs. "kills," "was killing" vs. "killed," "has been killing" vs. "has killed," "had been killing" vs. "had killed"; (E) POTENTIAL or NON-POTENTIAL: "will kill" vs. "kills," "would kill" vs. "killed," "will have killed" vs. "has killed," "would have killed" vs. "had killed," "will be killing" vs. "is killing," "would be killing" vs. "was killing," "will have been killing" vs. "has been killing," "would have been

killing" vs. "had been killing" (I omit the other auxiliary verbs of the duplex series "will"—"shall" and "can"—"may" which likewise have but a preterit and a non-preterit form).[1]

The auxiliary verb "do," used in assertorial, verificative constructions—ostensible affirmation, "nexal negation," and "nexus-question" (Jespersen's, 1924, terms)—is not combinable with other auxiliary verbs and therefore the number of possible selections between (F) ASSERTORIAL and NON-ASSERTORIAL is substantially reduced "does kill" vs. "kills" and "did kill" vs. "killed."[2] Since any nexal negation and any nexus-question has a patently assertorial, verificative modality (a "verdictive" modality, according to Willard Quine's terminological suggestion), in these cases a simple verbal form ("kills," "killed") is compulsorily replaced by a "do" construction, and there is no two-choice situation, whereas the distinction between a confirmation and a simple positive statement requires a choice of one of two possible constructions—"the man does kill the bull" or "the man kills the bull," "he did kill" or "he killed." Thus the lack (or at least the quite unusual character) of such interrogative constructions as "killed he" or "read you" in the formal pattern of English has a semantic motivation.

A diagram may sum up this survey of selective verbal categories in personal positive constructions: of each two opposites the more specified, "marked" category is designated by a plus, and the less specified, "unmarked" one by a minus; parenthesized minuses indicate the non-existence of corresponding pluses.

The choice of a grammatical form by the speaker presents the listener with a definite number of bits of information. The compulsory character of this kind of information for any verbal exchange within a given speech community and the considerable difference of the grammatical information conveyed by diverse languages were fully realized by Franz Boas, thanks to his astonishing grasp of the manifold semantic patterns of the linguistic world:

> The aspects chosen in different groups of languages vary fundamentally. To give an example: while for us definiteness, number, and time are obligatory aspects, we find, in another language, location—near the speaker or somewhere else, source of information—whether seen, heard [i.e., known by hearsay], or inferred—as obligatory aspects. Instead of saying "The man killed the bull," I should have to say, "This man (or men) kill (indefinite tense) as seen by me that bull (or bulls)" (Boas 1938:133).

Those who might tend to draw inferences of culture from a range of grammatical concepts are immediately warned by Boas: aspects that must be expressed may be abundant in some languages, and sparse in others but "a paucity of obligatory aspects does not by any means imply obscurity of speech. When necessary, clarity can be obtained by adding explanatory words." To denote time or plurality, those languages which have no tense or grammatical number resort to lexical means. Thus the true difference between languages is not in what may or may not be expressed but in what must or must not be conveyed by the speakers. If a Russian says: *Ja napisal prijatelju* "I wrote a friend," the

| Verbal Form | Selective Categories | | | | | |
|---|---|---|---|---|---|---|
| | A | B | C | D | E | F |
| kills | − | − | − | − | − | − |
| killed | − | + | − | − | − | − |
| has killed | − | − | + | − | − | (−) |
| had killed | − | + | + | − | − | (−) |
| will kill | − | − | − | − | + | (−) |
| would kill | − | + | − | − | + | (−) |
| will have killed | − | − | + | − | + | (−) |
| would have killed | − | + | + | − | + | (−) |
| is killing | − | − | − | + | − | (−) |
| was killing | − | + | − | + | − | (−) |
| has been killing | − | − | + | + | − | (−) |
| had been killing | − | + | + | + | − | (−) |
| will be killing | − | − | − | + | + | (−) |
| would be killing | − | + | − | + | + | (−) |
| will have been killing | − | − | + | + | + | (−) |
| would have been killing | − | + | + | + | + | (−) |
| does kill | − | − | (−) | (−) | (−) | + |
| did kill | − | + | (−) | (−) | (−) | + |
| is killed | + | − | − | − | − | (−) |
| was killed | + | + | − | − | − | (−) |
| nas been killed | + | − | + | (−) | − | (−) |
| had been killed | + | + | + | (−) | − | (−) |
| will be killed | + | − | − | (−) | + | (−) |
| would be killed | + | + | − | (−) | + | (−) |
| will have been killed | + | − | + | (−) | + | (−) |
| would have been killed | + | + | + | (−) | + | (−) |
| is being killed | + | − | − | + | − | (−) |
| was being killed | + | + | − | + | − | (−) |

distinction between the definiteness and indefiniteness of reference ("the" vs. "a") finds no expression, whereas the completion of the letter is expressed by the verbal aspect, and the sex of the friend by the masculine gender. Since in Russian these concepts are grammatical, they cannot be omitted in communication, whereas after the English utterance "I wrote a friend," interrogations whether the letter has been finished and whether it was addressed to a boy-friend or to a girl-friend, can be followed by the abrupt reply—"it's none of your business."

Grammar, a real *ars obligatoria*, as schoolmen used to call it, imposes upon the speaker its yes-or-no decisions. As Boas repeatedly noticed, the grammatical concepts of a given language direct the attention of the speech community in a definite direction and through their compelling, obtrusive character exert an influence upon poetry, belief, and even speculative thought without, how-

ever, invalidating the ability of any language to adapt itself to the needs of advanced cognition.

Besides those concepts which are grammaticalized and consequently obligatory in some languages but lexicalized and merely optional in others, Boas descried certain relational categories compulsory all over the world: "the methods by means of which these . . . relations are expressed vary very much but they are necessary elements of grammar." Here belongs, for instance, the distinction between subject and predicate, and between predication and attribution, as well as grammatical reference to the addresser and addressee. This problem of indispensable, universal categories in grammar was outlined by Boas and his inquisitive disciple Sapir (1921) in defiance of the neogrammarian aversion to any search for universals, and is crucial for linguistics today.

What aspects of information are obligatory for any verbal communication all over the world and what others only for a certain number of languages was for Boas the paramount question which divided universal grammar from the grammatical description of single languages and which, furthermore, enabled him to draw a demarcation line between the domain of morphology and syntax with their compulsory rules and the freer field of vocabulary and phraseology. In English, as soon as one uses a noun, the two choices—one between plural and singular and the other between definite and indefinite—are necessarily made, whereas in an American Indian language having no grammatical devices for number and definiteness, the distinction between "the thing," "a thing," "the things," and "things" may be either simply passed over or deliberately supplemented by lexical means.

It was clear to Boas that any difference of grammatical categories carries semantic information. If language is a tool serving to convey information one could not describe the constituent parts of this instrument with disregard of their functions, as a description of an automobile without any reference to the tasks of its working parts is incomplete and inadequate. Boas never gave up the key question: what is the informational difference between the grammatical processes observed? He would not accept a nonsemantic theory of grammatical structure, and any defeatist allusion to the imaginary obscurity of the notion of meaning seemed to Boas itself obscure and meaningless.

His work with native informants, in particular with his long-term guest from a Kwakiutl tribe, discloses Boas' scrupulous and objective approach. He intently observed how the unusual New York experience of the Indian crossed with the native pattern. In conversation Boas loved to depict the indifference of this man from Vancouver Island toward Manhattan skyscrapers ("we build houses next to one another, and you stack them on top of each other"), toward the Aquarium ("we throw such fish back in the lake") or toward the motion pictures which seemed tedious and senseless. On the other hand the stranger stood for hours spellbound in the Times Square freak shows with their giants and dwarfs, bearded ladies and fox-tailed girls, or in the Automats where drinks and sandwiches appear miraculously and where he felt transferred into the universe of Kwakiutl fairy-tales. In the same way, his whimsical tangling

of the Indian vernacular and English presented Boas with invaluable clues to the particularities of Kwakiutl grammatical concepts.

The bilingual equations, but first and foremost the interpretation of these concepts through equivalent expressions, is precisely what linguists understand by "meaning" and what corresponds to Charles Peirce's (1934) semiotic definition of a symbol's meaning as its translation into other symbols. Thus meaning can and must be stated in terms of linguistic discriminations and identifications, just as, on the other hand, linguistic discriminations are always made with regard to their semantic value. The responses of speakers to their language or, as one could say now—"metalinguistic operations," are equational propositions which arise as soon as there is uncertainty whether both interlocutors use the same verbal code and about how far one's utterance is understood by the other. Such metalinguistic interpretations of a message through paraphrases, synonyms or through actual translation into another language or even into a different set of signs, play a tremendous role in any process of language learning whether by infants or by adults. These equational propositions occupy an important place in the whole corpus of utterances, and, along with all other specimens from a given corpus, they may be submitted to distributional analysis (is it said, and in what contexts, that "A is B," that "B is A" and/or that "A is not B" and "B is not A"?). Thus the strictly linguistic technique of distributional analysis appears to be wholly applicable to problems of meaning both grammatical and lexical, and meanings can no longer be regarded as "subjective intangibles." The elicitation of meanings through metalinguistic operations of native speakers is a more reliable and objective device than the appeal to these natives for an evaluation of sentences in regard to their acceptability. Ellipses or anacolutha, inadmissible in an explicit and punctilious style, could be easily condemned by an informant despite their colloquial, emotive, or poetic use.

Chomsky has made an ingenious attempt to construct a "completely nonsemantic theory of grammatical structure." This intricate experiment is actually a magnificent *argumentum a contrario*, particularly useful for the pending inquiry into the hierarchy of grammatical meanings. The examples brought to discussion in Chomsky's *Syntactic Structures* (1957) may serve as interesting illustrations to Boas' delimitation of the grammatical class of meanings. Thus parsing the allegedly nonsensical sentence "Colorless green ideas sleep furiously" (1957:15) we extract its pluralized topic "ideas," said to develop a "sleeping" activity, and both terms are characterized—the "ideas" as "colorless green" and the "sleep" as "furious." These grammatical relations create a meaningful sentence which can be submitted to a truth test: do things like colorless green, green ideas, sleepy ideas, or a furious sleep exist or not? "Colorless green" is a synonymous expression for "pallid green" with a slight epigrammatic effect of an apparent oxymoron. The metaphoric epithet in "green ideas" is reminiscent of Andrew Marvell's famous "green thought in a green shade" and of the Russian idiom "green boredom" (*zelenaja skuka*) or of Tolstoj's "horror red, white and square" (*Vse tot zhe uzhas krasnyj, belyj, kvadratnyj*). In

its figurative sense the verb "sleep" means "to be in a state like sleep, as that of inertness, torpidity, numbness," e.g., "his hatred never slept"; why, then, cannot someone's ideas fall into sleep? And, finally, why cannot the attribute "furious" emphatically render a frenzy of sleep? Dell Hymes actually found an application for this sentence in a senseful poem written in 1957 and entitled "Colorless Green Ideas Sleep Furiously."

But even if we pedantically censor any image-bearing expression and deny the existence of green ideas, also then, as in the case of "quadrature of the circle" or "pigeon's milk," the nonexistence, the fictitiousness of these entities has no bearing on the question of their semantic significance. The possibility of questioning their being is the best warning against a confusion of ontological irreality with senselessness. There is, furthermore, no reason for assigning to the kind of constructions here discussed "a lower degree of grammaticalness." In a comprehensive dictionary of Russian the adjective signifying "pregnant" was labeled *femininum tantum* because—*beremennyj muzhchina nemyslim* "a pregnant male is inconceivable." This Russian sentence, however, uses the masculine form of the adjective, and the "pregnant male" appears in folk-legends, in newspaper hoaxes and in David Burliuk's poem: *Mne nravitsja beremennyj muzhchina, prislonivshijsja k pamjatniku Pushkina* "I like the pregnant man leaning against the monument of Pushkin." The masculine occurs, moreover, in a figurative use of the same adjective. Similarly, a French girl in a primary school claimed that in her mother tongue not only nouns but also verbs have their gender, e.g., the verb *couver*, hatch, is feminine, since "hens hatch but roosters don't." Nor for grading levels of grammaticalness can we use ontological argument and exclude the pretended "inverse non-sentences" like "golf plays John" (Chomsky 1957:42; cf. such perspicuous utterances as "John does not play golf; golf plays John").

Actual agrammaticalness deprives an utterance of its semantic information. The more the syntactic forms and the relational concepts which they carry appear to be obliterated, the less feasible is the truth test of the message, and phrasal intonation alone holds together such *mots en liberté* as "silent not night by silently unday" (e.e. cummings) or "Furiously sleep ideas green colorless" (N. Chomsky). An utterance such as "It seems to move toward the end" in the agrammatical version "Move end toward seem" can hardly be followed by a question: "Is it true?" or "Do you really mean it?" Thoroughly degrammaticalized utterances are nonsensical indeed. The constraining power of the grammatical pattern, recognized by Boas and opposed by him to our relative freedom in word selection, becomes particularly manifest through a semantic inquiry into the field of nonsense.

NOTES

[1] Neither progressive perfect nor progressive potential are used in passive, because two non-finite forms of the auxiliary verb "to be" are incompatible.

[2] Beside indicative, this auxiliary verb is used in imperative constructions only: "do kill!" vs. "kill!"

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## *Some Central Elements in the Legacy*

LESLIE SPIER

BOAS LEFT no body of dogma as a legacy. What he established, as a foundation to modern anthropology, was a series of guiding principles for action. These were expressed in concrete contributions, with little phrasing of theoretical points in extended form. Hence our survey of central elements here must stay close to the specific as he presented it.[1]

The life of Boas coincided with the establishment of anthropology as a discipline of definite scope and method. He, more than any other individual, can be credited with determining the nature of its field and giving it the scientific approaches of objective empiricism, carefully controlled analysis, firmness of aims, and scrupulous self-discipline in defining the axioms of one's thoughts. For all he maintained that he was merely analyzing the fundamental views of his predecessors, so far as anthropology is a science he made it one.

Boas was perhaps the last man who can be said to have embraced the whole field of anthropology. Since his earlier days the field has become so ramified, and demands so intimate a knowledge of some segment, that no one can encompass it again in so masterly a fashion. Yet a fundamental of our heritage from him is that the wholeness of culture and the unity of problems of physical types must be borne in mind while dealing with particulars.

To appreciate the extent of his contribution, we must bear in mind that at the time he came into anthropology in the 1880's and for some decades after, it was given over largely to speculation and generalization—and this is not to deny that early writers had given it direction, some formulation of problems, and had tried to create a methodology. Now that its premises have been established for us, we tend to assume that the points were obvious and needed no demonstration, forgetting that at the opening of Boas' career quite other views were held as equally obvious with unshakable conviction. What he did, in contrast to his predecessors, was to formulate our problems in specific form and provide basic valid methodology. That there are now many more problems and approaches does not absolve us from remembering the circumstances in which the axioms were laid down.

His first field experience (1883–84), among the Baffinland Eskimo, set the pattern of his thinking. He confessed that he went with a crass belief in environmental compulsions of Eskimo life and thought, but his stay among them revolutionized his viewpoint. He then became convinced, he said, that while there was an adjustment of life to external physical conditions, social tradition—the result of a multiplicity of historical factors—was by far the more potent determinant of a man's thought and behavior. This was the compelling idea of his life's work: the complete molding of every human expression—inner thought and external behavior—by social conditioning.

What then are the relations between the bodily constitution and endowment and the cultural matrix in which it exists? Boas' career turned fundamentally on the relations between the two. Much that is basic is summed up in his *The Mind of Primitive Man* (1911c), a book which has had a profound influence on the thought of our generation—not narrowly on anthropologists. but on all social-minded thoughtful individuals. It has become part of the heritage of all educated persons. Perhaps its import is better understood by noting the title of the German edition, *Kultur und Rasse* (1914a). Where Boas would seem by the English title to have concerned himself with savage mentality alone, actually the book's aim so far transcended that as to make it a Magna Carta of race equality—or better, of the equivalence of hereditary endowment among races and the independence of cultural achievement from race. It is quite true that for decades a more enlightened attitude had been emerging in the Western world, but it was Boas who wrote quietus to any "scientific" pronunciamentos on "higher" and "lower" races.

Several main themes in the book are now axiomatic for us. (1) No racial group today can lay substantial claim to hereditary purity. (2) Races are not stable entities, immutably fixed in early times, but show evidences of change due to domestication, environmental influences, selection, and perhaps mutation. (3) The average differences in physical characteristics between races is small in contrast to the great overlapping of range and duplication of types among them. (4) There is no fixed relation between function and anatomy, between mind and brain for example, such as to warrant the view that any race is incapable of participating in any culture or even in creating it. On the side of cultural analysis Boas demonstrated (5) that there is no identity of race, language, and culture such that physical heredity can be credited with the formulation of languages and the achievements of civilizations. (6) On the contrary, there is overwhelming evidence that these aspects of human behavior are the products of involved historical growth, though they rest on fundamental traits of mind and body, traits probably uniformly distributed among all races.

This emphasis on historical determinants is the keynote to Boas' thinking about culture, and it is our legacy. Applied specifically to primitive men, he concluded that such groups are not primitive by reason of hereditary inferiority but because the circumstances of their life were more static than those of civilized men, the differences being products of their variant history and traditional equipment. Such a formulation disposed of the belief of an older generation in cultural evolution (or parallelism): that each race is primarily responsible for its own development; that cultural evolution (perhaps racial and cultural evolution going hand in hand) passes through the same sequences everywhere, with savages left behind on an inferior level (perhaps because of incomplete biological development).

It should be clear that by "historical" Boas meant only that each cultural trait and configuration must have had a specific antecedent form. This did not involve the need to provide a sweeping picture-in-time (though that appeals

to some anthropologists of different temperament) or knowledge of which individuals brought about innovations. It sufficed for his purpose to envisage a "before and after" picture at a particular place and time. Cultures, obviously, are continually in a state of flux, whether slow or rapid; hence cultural forms evolve endlessly out of antecedent cultural forms. In this sense—and in this sense only—would one say that Boas was a cultural evolutionist—as we all are.

But such historical unravelling was not an end in itself. Boas was primarily interested in the processes of culture growth revealed in this historical panorama. Whether universals could be phrased with respect to processes, forms, and interrelations (functions) was a matter he approached with caution—and to the extent that we are like-minded, that caution, too, is a heritage. He wrote:

> . . . Certain laws exist which govern the growth of culture, and it is our endeavor to discover these laws. The object of our investigation is to find the *processes* [italics his] by which certain stages of culture have developed. The customs and beliefs themselves are not the ultimate objects of research. We desire to learn the reasons why such customs and beliefs exist—in other words, we wish to discover the history of their development. The method which is at present [1896] most frequently applied in investigations of this character compares the variations under which the customs or beliefs occur and endeavors to find the common psychological cause that underlies all of them. I have stated that this method is open to a very fundamental objection.
>
> We have another method, which in many respects is safer. A detailed study of customs in their bearing to the total culture of the tribe practicing them, and in connection with an investigation of their geographical distribution among neighboring tribes, affords us almost always a means of determining with considerable accuracy the historical causes that led to the formation of the customs in question and to the psychological processes that were at work in their development (1896:5).

An explanation is in order here—which is by no means a digression but significant for understanding this and comparable statements. Normally today the word "psychological" will evoke thought of the findings of modern experimental psychology (which hardly existed in 1896 and for many years after). Boas used the word in its older and more general sense alternatively with "mental." He had in mind always the inner states and mental content which are the realities of cultural habits and give meaning to their adherents—an aspect of the cultural whole which was either ignored or guessed at by his contemporaries.

Boas did not conceive his task as narrowly historical: he was concerned fundamentally with culture constructs as mental products. But where his predecessors sought general laws of psychic action by illusively simple analysis, he redefined the problem as that of the extent and character of mental operations at each specific point in their historically ascertained sequences. It has sometimes been said, without warrant, that Boas denied the possibility of deriving laws of culture and of mental action. He never phrased so drastic a statement: indeed, his firm belief in the essential similarity of minds in all races at all periods, and in the essentially equivalent nature of cultures the world over,

presumed regularities of mental operation and of culture growth. Where he sought laws, if it were possible to formulate them, is indicated in a number of statements.

> While on the whole the unique historical character of cultural growth in each area stands out as a salient element in the history of cultural development, we may recognize at the same time that certain typical parallelisms do occur. We are, however, not so much inclined to look for these similarities in detailed customs but rather in certain dynamic conditions which are due to social or psychological causes that are liable to lead to similar results. . . . In short, if we look for laws, the laws relate to the effects of physiological, psychological, and social conditions, not to sequences of cultural achievement (1920:318).

He held firmly to the principle that such regularities must be explicitly demonstrated and could or should be phrased only within their limiting circumstances. This tenet is that of an ever-rigorous and cautious thinker: he feared a recrudescence of premature formulations. These features of the nature of laws as known to the natural scientist (of which Boas, as a physicist by training, was undoubtedly more fully aware than his sociologist critics) are far from those of the facile generalizations offered by early writers.

The need for a substantial factual basis for such demonstration led him to a carefully conceived series of studies of primitive life: ethnographies monumental in scope and in detail; texts and grammatical analyses of many American Indian languages; vast collections of folktales—not as trivia, but for their variant forms which could provide materials for historical and pyschological analysis. Primary in these efforts is the conviction that for a true picture of native action and thought the records must be made through the native tongue, or, if command of the native language is not feasible, in the form of dictated texts.

Boas' methodology and aims are well exemplified in his study of tales, a life-long interest. To understand these aims and the different temper of his approach to the subject, we must recall that at the close of the last century there were ardent advocates of the view that the essential unity of men's minds and fancies inevitably produced like tales the world over, and equally ardent proponents that such tales could only have spread from some early centers. From the beginning he insisted that "in order to investigate the physical laws of the human mind . . . we must treat the culture of primitive people [here the tales] by strict historical methods. We must understand the process by which the individual culture grew before we can undertake to lay down laws by which the culture of all mankind grew." In *Dissemination of Tales among the Natives of North America* (1891), he laid down a test for historical unity: namely, that the tales compared must contain the same arbitrary elements and be part of a continuous area of distribution. In his *Growth of Indian Mythologies* (1896), he demonstrated that the myths and tales on the Northwest Coast were reintegrated forms of unit tales which crossed and recrossed in the course of their diffusion; in short, that the tales were intricate historic constructs, not simple immediate products of minds at play with fancy. On a very much larger scale,

he provided in the comparative notes to *Kutenai Tales* (1918)—an encyclopedic index of North American tales to the date of its publication—materials for a much broader approach to the same problem. Again, in reaction to the old rationalistic view that mythological tales arose from the anthropomorphizing of nature, he showed that, by a transfer of characters, the same tale was told of human, animal, and nature actors in adjacent areas. So too, that the explanatory (etiological) elements were in many cases not primary but later accretions to existing tales. These concepts of historically secondary connection and the reassembling and reintegration of existing elements played a significant part, not only in his view of the growth of mythologies but in Boas' whole conception of the history of culture processes.

Something of these discriminations between the historically determined and the universally human appears further in his folktale analyses. In *Stylistic Aspects of Primitive Literature* (1927b) he pointed out that certain features must have been historically determined: the moralizing fable, proverb, and riddle of Old World folk literature in contrast to their rare occurrence in the New, the tendency to cluster unit tales in long narratives or cycles in some areas against its absence in others, the specific motivations given locally to tales in conformity with cultural interests, and so on. On the other hand, general mental traits could account for such things as rhythmic repetition as a literary device to heighten interest, the tendency to make use of formal elements at the expense of free narrative, and, indeed, the impulses to associate and coordinate tales of diverse historical origin and to rephrase them in terms of local culture, its motivations, and stylistic norms. Such norms for large areas were presented in *Mythology and Folk-Tales of the North American Indians* (1914b). He attacked the same problems on an extensive scale in his *Tsimshian Mythology* (1916c) and again in *Kwakiutl Culture as Reflected in Mythology* (1935b), where by a very thorough inspection of a vast body of material, he gave answers to the questions how far—and in what ways—the culture of narrators determined content and form of the tales, what emotions and motives were expressed and how, and what formal elements or literary devices were utilized.[2] His thought was that a series of such studies would enable us to discriminate the culturally determined and that residual part which could be ascribed to the free play of fancy. Parallel analyses have since been made, but it must be confessed that present-day anthropologists show little interest in the marked possibilities of this approach—or in anything else pertaining to tales.

Coupled with this was Boas' interest in the interplay of imagination and convention in the graphic arts and song: indeed, they are all treated as aspects of the same fundamental activity in his *Primitive Art* (1927a). He developed the thesis that some esthetic elements resulted from technical control, which with developing skill resulted in play with technique; that virtuosity itself became a source of esthetic gratification; that rhythmic repetition and balance of design resulted frequently from regularity of the craftsman's movements. In an analysis of *The Decorative Art of the Indians of the North Pacific Coast* (1897), Boas had shown that the pseudo-realistic carvings of that area were strictly limited

in their execution by fixed conventions of art derived from historical developments in their past. These treatments were by no means the purely formal, atomistic dissection they may seem, for he understood full well that in each art there is a set of values—values of emotional concern to artist and beholder. But he attempted to unravel that with which most students of art are less concerned—the nature of such cultural constructs, the inventive or assimilative character of men's minds, and hence, the processes of culture growth. Here again are a set of leads waiting for some scholar.

Nor should it be thought that this emphasis on history and process left unregarded the individual in culture. What else was his basic concern with the attempt to discover the manner in which an individual's thought and activities become molded? Too great an emphasis on the historical might end in neglect of the carriers of culture. He wrote: ". . . the dynamics of social life can be understood only on the basis of the reaction of the individual to the culture in which he lives and of his influence upon society. . . . An error of modern anthropology, as I see it, lies in the overemphasis on historical reconstruction, the importance of which should not be minimized, as against a penetrating study of the individual under the stress of the culture in which he lives" (1930, in 1940:268–69). Unquestionably the overwhelming mass of an individual's overt activities are culture-channeled, but how far does this hold for states in which affect is uppermost—emotions, temperamental biases, sentiments. What, he asked, are the nascent physiological bases and what forms do these affective states take? Although at no point did Boas make the culture-moulding of the individual the sole objective of a study, there is material for it embedded in his ethnographies and in observations expressed in some more general papers; and, quite as significantly, he stimulated such studies among his students—for example, the study of adolescent behavior in differing cultural settings. Yet he was wary of "personality studies."

Some of our most familiar working concepts are a heritage from Boas, though commonly not recognized as such. The concept of the culture area first took form in his arrangement on a geographic basis of the exhibits at the World's Fair, Chicago, in 1893. When called to the American Museum of Natural History in 1895 he again embodied the idea in exhibits, where by display and label the concepts of areas of characterization, of typical and marginally variant cultures, were elaborated (and later developed and publicized by Wissler). Boas made no fetish of it: for him it served largely as a classificatory device for handling large bodies of data, but its utility for the analysis of the historical interrelations implied in such classifying has long been taken as a matter of course.

The legacy of Boas' long-time interest in language is not alone the great body of texts and grammatical analyses of previously unrecorded native tongues made by himself, his students, and their successors, but some general principles. What is axiomatic today—that each language must be viewed in terms of its own structure and operations—was a revolutionary proposition in the days when exotic languages were strained on the Procrustean frame of Old

World "literary" languages. Boas was not concerned with linguistic analysis as an end in itself, that is, as a sort of mathematical play—as it is (quite properly) for some linguists. For him language is a cultural form, the components of which are to be investigated as is any other culture manifestation. One novelty of his presentations are the sections devoted to "Ideas Expressed by Grammatical Processes," an attempt to understand the weighting of grammatical forms for classifying experience. These observations are of inestimable value to the cultural anthropologist, and we can only wish that linguists of today would give us further insights of this nature.

His general viewpoint is expressed in the introduction to the *Handbook of American Indian Languages* (1911b) and the inaugural article in the International Journal of American Linguistics (1917). There he stressed the need of language study for an understanding of the more subtle aspects of a culture, its verbal forms and nuances of social behavior, and the analysis of grammatical categories for an appreciation of the formal verbal framework circumscribing expressions of thought. At the same time he discriminated carefully lest the logic of language structure be mistaken for the logic of thinking. Grammatical categories, he held, are not the categories of thought, but they constrain the logic of thinking in certain channels.

In his introductory article for the Journal he made perhaps his strongest and most unique methodological point: he insisted that the first historical problem with respect to languages was to discover how far lexical, morphological, and syntactic features had been incorporated in a language from diverse sources before assuming genetic connection. Pointing out that "In America we can discern various areas that have common phonetic characteristics . . . [which] do not coincide with any morphological groupings, and are apparently geographically well defined . . . , [while] certain morphological types have a wide continuous distribution," caution is necessary before positing the similarities as the consequence of genetic relationship. "It is quite inconceivable that similarities such as exist between Quilleyute, Kwakiutl, and Salish, should be due to mere accident, or that the morphological similarities of Californian languages, which Kroeber and Dixon have pointed out, should not be due to a definite cause. The experience of Aryan studies might induce us to agree that these must be members of single linguistic stocks; but this assumption leaves fundamental differences unaccounted for, and neglects the possibility of morphological assimilation, so that at the present time the conclusion does not seem convincing. . . . It is not safe to disregard the possibility of a complex origin of linguistic groups." Boas' insistence on rigor of demonstration—in contrast to the quick apperception by persons with a somewhat intuitive grasp of far-reaching similarities not easily conceptualized—may not have been wholly advantageous but did inevitably flow from his scientific habits of thought. If this was a somewhat negative attitude—but a healthy one—a more positive suggestion followed: "It seems probable that a safer basis will be reached by following out dialectic studies" which promise "rich returns in the field of the mechanical processes of linguistic development and of the psycho-

logical problems presented by languages of different types" (1917:3–5). Some dialectic studies have since been made, but rarely brought to any head with general statements of the processes involved in dialect differentiation.

Quite as fundamentally a part of our heritage were Boas' contributions to understanding the nature and composition of bodily types. His concern throughout was with questions of racial strains, their homogeneity and hereditary characteristics; how, in the course of growth, physical characteristics are established; and by inference, the bearing of similarity or difference of physical forms on hereditary mental endowment, and what its consequences for cultural behavior might be. Year after year he picked up these problems, systematically moving them toward definite conclusions.

Methodology here centered in the analysis of mass data by statistical measures. He was not only a master of statistical analysis but an originator of new forms (see inter alia his mathematical treatise *The Measurement of Variable Quantities* [1906]). It is patent that many of his contemporaries in physical anthropology, unused to mathematical thinking, were bewildered, and something of this unawareness of the utility of statistical devices persists.

A basic contribution was his demonstration of the plasticity of bodily types. Growth, commonly studied only as that of childhood and adolescence, was seen as a process continuing through adult life to death, a continuum of change. "The life span is the result of physiological processes that go on throughout life and that have been observed from the time of birth until death. When we study the distribution of moments of occurrence of definite physiological changes, it appears that the variability in time of occurrence increases with great rapidity during life" (1935a, in 1940:89). The genesis of this view lay in the prior observation that mentally precocious children had better physical development than their age-mates, dull children the reverse. As mathematician Boas saw in the seriations of physical measurements for a given age the distributions characteristic of scattering of a group caused by a multiplicity of factors (the product of chance as portrayed in the probability curve). His interpretation was that this systematic scattering resulted from varying degrees of retardation and acceleration; that there was a close correlation between the several physical measurements and between these and mental status; and that this correlation varied systematically during the years of growth.

He saw another corollary. If the course of the child's physical development was determined by the exigencies of growth, it should be clear that the final adult form is not fixed by heredity alone but by somatic factors as well; and should such circumstances affect a local population as a whole, there should be not only evidence of the instability of racial types as we find them, but the whole set of assumptions of anthropometrists—that in measuring adults they were describing hereditary forms alone—would be subject to fundamental revision. Confirmation of the hypothesis came with the study of *Changes in Bodily Form of Descendants of Immigrants* (1911a) wherein were found characteristic differences between immigrant parents and their children born in

the United States, differences which were substantial, correlated with the length of residence of the mother, and persistent throughout life. An inspection of earlier studies showed parallel changes in local racial types of Europe and elsewhere to have been established but their significance overlooked. Further studies, e.g. on Italians and Puerto Ricans (1913: summed up in *New Evidence in Regard to the Instability of Human Types* [1916a]), confirmed the conclusion, as did the less extended studies of others prompted by these findings. It is important to note that Boas held that these observations implied only a limited plasticity of human types. But however limited, he did demonstrate that we cannot assume that measurements of a local group immediately reveal fixed hereditary characteristics.

Equally important for understanding the nature of racial types were his analyses of component strains in local populations. His analysis turned *On the Variety of Lines of Descent Represented in a Population* (1916b). He established that homogeneity within a racial group is not identical with purity of descent; that in such a group variability of families may well be small because each family represents all ancestral strains, while within families a high degree of variation may be predicted. This is a methodological point of importance for the study of local races, the consequences of in- or out-breeding, and genetic stability.

On these bases he could logically have little interest in the classification of races. The local intrabreeding group was his unit of analysis, and the emphasis was on discriminating hereditary contribution and environmental influences. Only when the biological histories of such groups became known would it be reasonable to arrange them in taxonomic order. This is obviously parallel to the basic principles of his treatment of the problems of culture and language.

In many ways the most fundamental of Boas' contributions was the rigor of scientific method: careful analysis, caution, and convincing demonstration.

Fashions change in anthropology; it is always more pleasant to graze in new pastures; it is far simpler to narrow to a specialty than to harass oneself with concern for the whole. Often enough we become absorbed in some minor segment, yet scientific rigor should obligate us, like Boas, to give consideration to interrelations with other aspects of culture and bodily form.

Much of Boas' legacy is now central in the corpus of beliefs of present-day anthropology, but some elements lie neglected awaiting the day when they will be the subject of renewed appreciation.

## NOTES

[1] A substantial part of the present article appeared in Spier 1943.

[2] For an ampler analysis see Spier 1931.

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Appended hereto are a list of sponsors whose donations are an expression of the continued importance of Boas to American anthropologists and a token of the hold his memory has upon us.

W. G.

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